Vol. 28, No. 6 PSYCHOLOGICAL REVIEW PUBLICATIONS June, 1931

Psychological Bulletin

EDITED BY

EDWARD S. ROBINSON, YALE UNIVERSITY

HOWARD C. WARREN, PRINCETON UNIVERSITY (*Review*)
SAMUEL W. FERNBERGER, UNIV. OF PENNSYLVANIA (*J. of Exp. Psych.*)
WALTER S. HUNTER, CLARK UNIVERSITY (*Index*)
HERBERT S. LANGFELD, PRINCETON UNIV. (*Monographs*)

WITH THE CO-OPERATION OF

G. W. ALLPORT, HARVARD UNIVERSITY; J. E. ANDERSON, UNIVERSITY OF MINNESOTA; C. M. DISERENS, UNIVERSITY OF CINCINNATI; W. T. HERON, UNIVERSITY OF MINNESOTA; J. A. McGEOCH, UNIVERSITY OF MISSOURI; K. S. LASHLEY, UNIVERSITY OF CHICAGO; J. T. METCALF, UNIVERSITY OF VERMONT; R. PINTNER, COLUMBIA UNIVERSITY.

CONTENTS

PUBLISHED MONTHLY (EXCEPT AUGUST AND SEPTEMBER)
FOR THE AMERICAN PSYCHOLOGICAL ASSOCIATION
BY THE PSYCHOLOGICAL REVIEW COMPANY
PRINCETON, N. J.

Entered as second-class matter at the post-office at Princeton, N. J., with an additional entry at Albany, N. Y.

Publications of the American Psychological Association

PSYCHOLOGICAL REVIEW

containing original contributions only, appears bi-monthly, January, March, May, July, September, and November, the six numbers comprising a volume of about 540 pages.

PSYCHOLOGICAL BULLETIN

containing critical reviews of books and articles, psychological news and notes, university notices, and announcements, appears monthly (10 numbers), the annual volume comprising about 720 pages. Special issues of the BULLETIN consist of general reviews of recent work in some department of psychology.

JOURNAL OF EXPERIMENTAL PSYCHOLOGY

containing original contributions of an experimental character, appears bi-monthly, February, April, June, August, October, and December, the six numbers comprising a volume of about 600 pages.

PSYCHOLOGICAL INDEX

is a compendious bibliography of books, monographs, and articles upon psychological and cognate topics that have appeared during the year. The INDEX is issued annually in June, and may be subscribed for in connection with the periodicals above, or purchased separately.

PSYCHOLOGICAL ABSTRACTS

appears monthly, the twelve numbers and an index supplement making a volume of about 700 pages. The journal is devoted to the publication of non-critical abstracts of the world's literature in psychology and closely related subjects.

PSYCHOLOGICAL MONOGRAPHS

consist of longer researches or treatises or collections of laboratory studies which it is important to publish promptly and as units. The price of single numbers varies according to their size. The MONOGRAPHS appear at irregular intervals and are gathered into volumes of about 500 pages.

JOURNAL OF ABNORMAL AND SOCIAL PSYCHOLOGY

appears quarterly, April, July, October, January, the four numbers comprising a volume of 448 pages. The journal contains original contributions in the field of abnormal and social psychology, reviews, notes and news.

ANNUAL SUBSCRIPTION RATES

Review: \$5.50 (Foreign, \$5.75). **Index:** \$4.00 per volume.
Journal. \$6.00 (Foreign, \$6.25). **Monographs:** \$6.00 per volume (Foreign, \$6.30).
Bulletin: \$6.00 (Foreign, \$6.25). **Abstracts:** \$6.00 (Foreign, \$6.25).
Abnormal and Social: \$5.00 (Foreign, \$5.25). Single copies \$1.50.
Current numbers: **Review** or **Journal**, \$1.00; **Abstracts**, 75c; **Bulletin**, 60c.

COMBINATION RATES

Review and Bulletin: \$10.00 (Foreign, \$10.50).
Review and J. Exp.: \$10.00 (Foreign, \$10.50).
Bulletin and J. Exp.: \$11.00 (Foreign, \$11.50).
Review, Bulletin, and J. Exp.: \$15.00 (Foreign, \$15.75).
Review, Bulletin, J. Exp., and Index: \$18.00 (Foreign, \$18.75).

Subscriptions, orders, and business communications should be sent to the

PSYCHOLOGICAL REVIEW COMPANY

PRINCETON, N. J.

Vol. 28, No. 6 June, 1931

THE PSYCHOLOGICAL BULLETIN

THE ACQUISITION OF SKILL[1]

BY JOHN A. McGEOCH
University of Missouri

I. LEARNING—EXPERIMENTAL

The Contemporary Interest in the Problems of Learning. Over the last decade, or somewhat longer, interest in the problems of learning, both experimental and theoretical, has been steadily mounting. With increased interest have come many important changes in methodology, in the scope of problems, and in theoretical interpretations of results. There is scarcely a problem, if one except a very few, such, for example, as that of sex differences or of sensory modality in its narrower usage, which has not taken on new meanings and acquired new relationships with other problems. This progressive extension of the field of learning is making it perhaps the most intellectually exciting province of psychology, deserving of Weiss'(180) characterization as psychology's "outstanding problem."

This concern with learning is an important comment upon contemporary systematic thinking, particularly in this country where a

[1] This review covers the period since the preceding review of skill by the writer, "The Acquisition of Skill," PSYCHOLOGICAL BULLETIN, 1929, 26, 457–498. It is confined to experimental and theoretical work which bears directly upon the problems of learning as such. A great many related materials upon such topics as training in industrial skills, how to study, child training, the acquisition of academic subjects, testing of apititude for the acquisition of skills, and the like, have not been included. Only a few textbooks, which treat learning with some systematic thoroughness have been mentioned. In a field with extrinsic relationships as widely branched as are those of skill, it is often difficult to draw a defensible line between what bears on skill *per se* and what does not, and selection must be somewhat arbitrary.

major part of the work upon skill is being done. It constitutes a recognition of the systematic pervasiveness of the concept of learning and is an effort to make clear its implications. Many of the books and papers cited in this review point, either directly or indirectly, to this pervasiveness and there have been several explicit developments of it in connection with special fields. Carmichael (**18**) has ably surveyed the relations between the psychology of learning and the psychology of testing, and Kern's (**89**) whole book is an implicit working out of these relations. The rôle of learning in perceptual functions has been brought to a focus in several papers (Chou, **23**, and Renshaw, **135**, *e.g.*); learning grades so indistinguishably into reasoning that it is difficult to determine the boundary lines for the purposes of a review; and in many other directions its ramifications go far. It is even becoming apparent that an adequate theory of mind (or system of psychology, as one chooses) must wait upon an acceptable solution of some of the fundamental problems of learning.

The General Form of the Curve of Learning. Direct empirical studies of the general form of the learning curve have been almost lacking. The major studies of the curve have been dictated by the mathematical quest for adequate equations. The usual negatively accelerated form has appeared in the data upon many problems, and occasional instances of initial positive acceleration have been reported, as in the Henshaw and Holman (**63**) curves for chain assembling and in Smith's (**150**) dictation and copy curves for shorthand. The latter finding recalls Jette's (1928) results with Burnz shorthand. The vocabulary learning curves published by Strayer (**160**) for two identical twins are positively accelerated. Husband's (**83**) curves for maze learning, plotted on semi-logarithmic paper, approach linearity.

In an important paper on the learning function, Thurstone (**166**) develops a rational equation which agrees well with the empirical characteristics of learning, and offers a series of experimentally testable hypotheses, some of which are here briefly summarized. A homogeneous task will give a symmetrical S-shaped curve; while the more heterogeneous the task, the lower will be the inflection point. If a heterogeneous task has two levels of complexity, a plateau of certain predictable characteristics will result. Waning of the subject's interest moves the inflection point to the lower half of the curve; improving the method moves it to the upper half. "Insight" appears when the task contains a small number of psychological elements to be fixated or eliminated, and in cases of insight the errors are eliminated without being overtly made. If the individual learning

curves of a group are symmetrical and S-shaped, the composite curve will have a low inflection point.

Chaisson (20) calls attention to Robertson's view that the time relations of central nervous phenomena are governed by autocatalytic processes, and to the fact that the autocatalytic reaction equation fits growth curves well. Chaisson then proceeds to fit this equation to data from an experiment upon typing a selection of 1,668 strokes scored in strokes per minute. The accuracy of the fit suggests that "curves of this type are the upper limbs of S-shaped curves of autocatalytic nature." A more specific statement of the experimental conditions in this case would have assisted interpretation. In support of the general conclusion are offered the data for the relation between length of list and learning time. Such curves are, however, far from being comparable to those for continuous lists or skills and can scarcely be urged as evidence for the S-ness of the latter.

The usual linear method of plotting learning curves has certain limitations which Husband has analyzed: (A) the actual shape is determined by the spatial separation of successive units; (B) unit gains cannot be handled unambiguously; and (C) objective and decisive comparisons between curves are difficult. Equations, furthermore, are highly specialized and have only individual application. The method of plotting on semi-logarithmic paper meets these difficulties. Kern and Lindow (90) describe a graphical method for the determination of the best fit, and enunciate a law of practice to the effect that the mean values for successive periods in a practice series rise in the manner of a logarithmic function. A graphic method, presented by Valentine (168), is adapted to any curve fitting where several parameters are to be evaluated. This method reduces the time required from hours to minutes. Valentine (169) has also studied the relation between the hyperbola and the arc cotangent function and concludes that "for all positive values of x beyond some point slightly above zero, the arc cotangent function may be made to approximate the hyperbola closer than either curve will approximate the learning scores." Gariayeva (49) criticizes a formula used in 1928 by Heinis on the ground that it does not permit prediction of the limits of educability, and proposes an hyperbola with three parameters as representative of the "law of educability."

The Plateau Phenomenon. M. D. Smith (150) holds that the work of the last thirty years has established two negative results regarding plateaus: (A) they do not usually occur in the learning of a relatively simple process; and (B) even in complex processes they

do not necessarily occur. An experimental search for plateaus was made in the curves for target throwing, the guidance of a small metal a ball up an inclined plane and among holes by means of a knitting needle, and a phonetic system of shorthand. In addition to daily fluctuations, periods of arrested progress occurred. About 60 per cent of these are assigned to accidental factors, and the remainder to factors inherent in learning. From the curves and from introspections the conclusion is drawn that the causes of plateaus may be summarized as "difficulties of coördination," "a successful coördination seeming to involve a certain distribution of attention and a certain ratio of skill between the individual components." Difficulties tend to occur when the subject regards the task as composed of elements, but not when he envisages it as a whole.

The whole problem of the plateau phenomenon is, writes Hunter (**78**), in need of experimental clarification. He offers the hypothesis that "plateaus may be a genuine and necessary aspect of certain processes of learning and in other instances they may merely indicate the experimenter's failure to keep working conditions constant for his subject." Relying on Snoddy's (1926) results, Wheeler (**181**) interprets plateaus as a result of a poor distribution of stimulation and consequent irradiation patterns. Lack of attention, lowered interest and the like are symptoms of the irradiation, not causative factors.

Learning in Perceptual Functions. In several papers on perceptual functions learning has been one of the fundamental issues. Departing from the inference that, if cutaneous localization is learned, its accuracy should increase with practice, Renshaw (**134**) gave both children and adults extended training in the function. Large practice effects appeared. In a second paper by Renshaw, Wherry, and Newlin (**135**), in which congenitally blind subjects were compared with the seeing, significant practice effects in localization again were shown. The differences between children and adults, blind and seeing, are important rather for a theory of localization than for learning *per se*. These two papers, however, combine to bring the field of cutaneous space within the general field of learning.

In Ewert's (**42**) study of the effect of inverted retinal stimulation upon spatially coördinated behavior, learning was rapid where overt localizing responses were present, and less rapid when they were absent. It is interesting that learning consisted in a gradual decrease in the number of errors rather than a decrease in their average extent. This is apparently a case of all-or-none elimination. Other cases in

point of the significance of learning for perceptual activity are the work of Crosland, Taylor and Newsom (**30**) on curves for improvement from practice in the perception of the Müller-Lyer illusion; that of Dunford (**37**) on improvement with practice in the accuracy of cutaneous localization; G. G. Brown's (**13**) study of the perception of depth with disoriented vision; Guilford's (**58**) work on learning to read facial expression; and the work of Israeli (**84**) on the perception of short time intervals.

The Whole Method versus Part Methods. Crafts (**27, 28**) has made two new and significant attacks upon this problem. Previous experiments have dealt almost exclusively with serial reactions in a fixed order in which each response is the stimulus for the next. The inferiority of the part method has largely been accounted for by place associations, failure to master the acts of connection, and the like. If these explanations are valid, the pure part method should appear to better advantage with non-serial reactions which lack any predictable sequence. In his first experiment card-sorting into 9 compartments was employed and progress under the following methods was measured: (A) whole method, whereby the subject sorted for 10 consecutive trials a pack containing 8 each of 9 different cards; (B) pure part method, in which each part was a single row of 3 compartments; (C) combination part method, by which the subject was given one trial with each of 3 rows and then given combinations thereof; (D) progressive part, whereby the subject began with one row and added one compartment at each succeeding trial to the end of the sixth; and (E) the above methods with complex sorting in which cards numbered "2" go into the "1" compartment, and so on. In the part methods the last four of the ten trials were always by the whole method. In evaluating the results, the criteria used were speed attained after a given number of repetitions and the amount of practice required to attain a given speed. By the first criterion the whole method was superior to any part method and the combination and progressive part methods were equal to each other and superior to the pure part. By the second criterion, the whole method was again superior, especially for attaining the lower speeds. The pure part, plus the concluding trials by wholes, was superior to the other part methods plus concluding practice by wholes, for the attainment of the higher speeds. In complex sorting the whole method was superior by both criteria. Crafts reviews the possible explanations of these results and concludes that the superiority of the whole method comes

from the relationships in which the subject was able to coördinate the compartments when all were being reacted to in a trial.

Card-sorting obviously involves spatial relations. Crafts' second experiment used letter-digit substitution in which the spatial factors are unimportant. The whole, pure part, and combination part methods were compared under massed and spaced practice. With massed practice the methods are not far apart, but what differences there are favor the part methods. When practice is spaced, however, the whole method is consistently superior. Possible explanations are examined and the presence of less fatigue in the spaced-whole learning is suggested as an important reason for its superiority.

These two papers contain much valuable material not here summarized and are a refreshing approach to the old whole-part problem. In the interpretation of the results of the second experiment Crafts makes a statement which is symptomatic of the implications frequently drawn by other experimenters from their results. Criticism of this statement in no way reflects upon the paper from which it is taken and is made in this connection only because of the prevalence of the implication. Crafts suggests that the whole method is in some sense really superior and that distribution of practice permits this superiority to show itself. It seems to be implied that the whole method has an inherent, an absolute superiority, as, likewise, students of the problem of distribution have seemed to believe that distribution has an intrinsic advantage over massing, and as writers on the learning curve have written as if there were a *real* curve which could be discovered by proper methods and a sufficiently general equation. Such a point of view seems to the reviewer to run afoul of the nature of the phenomena and of the logic of science. Our experiments reveal the conditions under which a given method is best or a given curve appears, and the scientific goal can scarcely be other than a discovery of all possible functional relations. No phenomenon which science knows is unconditioned, and if absolutely best methods and real learning curves were suspected of existence, one would have to look for them in some heaven of ideas, not in the laboratory.

Beeby (5) points out a fundamental difference between the part-whole problem in verbal learning and in motor skills. The former is one-dimensional, has only successive constituents; the latter is two-dimensional, has simultaneous as well as successive constituents. His experimental work, which was upon a form of continuous tracing, involved both the transfer and the whole-part issues. He concludes that the effect of simultaneous combination upon a skilled action is

harmful if vision is excluded, but that the harmful effect disappears when vision is permitted. An experiment by Smith and Powers (**157**) upon the relative values of discrete vocabulary and sentence practice for language learning bears upon the part-whole issue. By two criteria the discrete word practice, a part method, was superior. This result is explained on two principles: (A) in sentence practice the interval between substitute stimulus and conditioned response is greater; and (B) in sentence practice there is more opportunity for inhibitory conditioning.

Distribution of Practice. Lorge (**105**) analyzes Snoddy's (1926) data and holds that Snoddy's conclusion that distribution conditions rate of improvement only during the early, adaptation phase of learning is not justified. In an extensive series of experiments Lorge uses the stabilimeter, mirror reading, nonsense syllables, and code learning with, in most experiments, 20 practices and interpolated intervals of zero, one minute and one day. Comparisons are made between achievement under massing and achievement at the same trial of another group working under distribution. The general superiority of distribution is substantiated and it is found, in addition, that at each succeeding practice the difference in performance is attributable to the interpolated time interval and not to other factors in the experimental situation. The limit of the contribution of distribution is the limit at which learning is reached. Two explanations of the beneficial effects of distribution are considered: (A) the interpolated interval allows the neural processes to "set"; and (B) intervening rest periods may produce greater satisfyingness and better attention. The results are interpreted in favor of the former.

A study has been made by Hardy (**61**) with human subjects paralleling, on the same maze pattern, Warden's (1923) work with animals. The groups used were equated on the basis of the first three trials and frequencies of 3 and of 5 trials were employed with rest intervals of 12 hours, 1, 2, 3 and 4 days. At a frequency of 3 trials, efficiency at first decreases and then consistently increases as the interval is varied from 12 hours to 4 days. The optimal interval is 4 days, while 1 day is the poorest. At a frequency of 5 trials, there is no conclusive evidence that efficiency is a function of the length of the interval. What indications there are favor the longer intervals. Nor is there definite evidence that speed of learning varies with the number of trials. These conclusions differ strikingly from those of Warden, and Hardy concludes that the optimal distribution varies with the type of organism studied. Kern's (**89**) work on six practice

Kern

tests leads him to formulate the principle that maximal efficiency is obtained from the shortest practice periods distributed over the longest possible time. The data of Henshaw and Holman (**63**) on chain assembling indicate again the ineffectiveness of relative concentration in comparison with a relatively greater distribution. Lashley's (1917) report, from a study of the learning of rats, that fewer duplicate errors appear in trials separated by an interval is corroborated by McGinnis' (**111**) work on the maze learning of young children.

The Influence of Special Motivation. Leuba (**103**) has published a discriminating analysis of the nature and effects of incentives. He gives an objectively stated list of the more common elementary incentives and points out that there are two aspects to an incentive: the incentive situation and the incentive attitude which it induces. In experiments upon the effect of incentives on learning no clue has been given to how much the subject would exert himself, if necessary, to secure the incentive offered. In another paper Leuba (**104**) criticizes previous experiments on the grounds that the experimenters have not known what incentives were present in the control group; in some of them care has not been taken to prevent communication between groups; and in some experiments members of the control group knew each other's level of performance, while in others they did not. He has performed a preliminary experiment to quantify an incentive and to measure its effect upon learning. Children, of average age 11.3 years, practiced 2-digit multiplication three 10-minute periods weekly for 7 weeks. The chief incentive situation studied consisted of chocolate bars which the subject received only if he did a given number of problems. This number increased in magnitude as practice progressed and was determined empirically so that approximately an equal number of slow, medium, and fast workers would receive the reward. Variations of this technique and other incentives, also, were studied. The chocolate bar incentive produced an increase of 52 per cent over the no-incentive level, which becomes the more significant when one remembers that multiplication was an old skill for these subjects.

A study of the influence of certain incentives upon the accuracy of discrimination on the Galton bar has been reported by Hamilton (**60**). Reward (bell rung if judgment right) and punishment (bell rung, if wrong) had an equally stimulating effect on accuracy. These "incentives" seem to be, however, as much modes of guidance or of effect as conditions of the incentive order. This is particularly

true of the conditions in which knowledge of results was given. From an examination of the influence of three intensities of electric shock upon maze learning, Vaughn and Diserens (170) conclude that "if the task is extremely simple and the methods of meeting it come within the individual's span of attention, punishment will increase the efficiency of its performance. If, on the other hand, the task is complex and the methods of meeting its various phases do not all come within the span of attention, certain of the methods will be selected and others will be neglected." This conclusion makes the interrelations of motivation, task and span of attention extremely important. In point, also, is Bowman's (10) study of the theory of interest in relation to learning capacity.

In a discussion of the influence of motivation upon learning, Wheeler (181) holds that drives are not special mechanisms acting on learning. On the other hand, these drives are tensions of varying degree which are resolved in the goal direction of least action. Feelings and emotions do not condition learning, but both emotion and learning are conditioned by the same underlying factors in such a fundamental way that the same behavior may often be described as either emotive or learning behavior, or as both.

The experimental work on the influence of knowledge of progress is difficult to classify because of the fact that it scarcely constitutes an independent problem, but divides, in significance, into at least three others. In comparisons between the control and knowledge groups, the question of the efficacy of sheer, uninformed frequency is raised; and the interpretation of the performance of the knowledge groups looks toward guidance, toward motivation, and toward effect. In view of the recent statements of the latter in terms of consequences, the work on this problem is really work on the law of effect. A few such studies will be mentioned here. In an investigation of the effect of written tests at each meeting of freshman classes in philosophy, Deputy (35) finds that such measures and knowledge of their results are of value. Likewise Panlasigui and Knight (124) report clear advantages from the use, with Grade IV pupils, of individual and class progress charts in arithmetic. When Hamilton (60) told his subjects the direction of their errors on the Galton bar, without the reward or punishment attitude, less improvement resulted than from either attitude without knowledge being directly given. The gains without knowledge, found by Crosland, Taylor and Newsom (30) will be returned to in connection with their theoretical implications.

The Influence of Guidance. Over a twelve-year period reports of

research upon guidance have been issuing from Carr's laboratory at Chicago and a large amount of data has accumulated in the journals and monographs. Carr (**19**) has greatly aided both interpretation and research by bringing this work together in a condensed summary which brings to a focus what is known of the laboratory relations between teaching and learning. Especially valuable is his interpretation of the theoretical bearing of the results, which will be reviewed later. A still further condensation of his summary will not be attempted. Some important unsolved problems are discussed in a way to "emphasize the significant fact that any attempt to teach an act of skill does involve a series of problems which we know very little about."

Prior work on guidance in the maze has been confined to 20 trials or less. Waters (**177**) has carried the frequency of manually guided trials far beyond this point in order to study the influence of guidance in very large amounts. Four groups of 20 subjects each were given either 0, 20, 40 or 80 manually guided trials requiring 20 seconds each, after which they completed the learning unaided. When the records subsequent to learning are considered, the efficacy of the guidance increases, by all criteria, with increases in the amount given, becoming relatively less with the larger amounts. When guidance is included as a part of the learning records, it is detrimental in terms of trials, less so in terms of errors. It also decreases final speed. The relations between these and previous results upon guidance are discussed. The work of Peterson and Allison (**132**) is closely related to that reported by Carr on visual guidance in 1921, but differs in that the stress is upon the influence of visual exposure on reliability as well as on rate. Four groups of subjects learned a complex stylus maze. One group was given no direct visual aid; the three others were allowed 5, 10 and 20 seconds, respectively, of visual exposure immediately before each trial. The three exposures facilitate learning in the order of their magnitudes and increase reliability. The errors in the odd-even trials correlate, for the 0, 5, 10 and 20 seconds exposures, 0.48, 0.75, 0.87, and 0.87, respectively. Visual exposure of this character evidently plays a dual rôle. Hamilton's (**60**) work, already mentioned under the head of motivation, relates also to guidance, since his incentive conditions were such as to teach the subjects regarding the required response. These conditions were effective. Ewert's (**42**) instructional guidance was a factor in establishing localization, and Freeman (**47**) reports that "insight" resulting from

instruction exerts a determining influence over the items to be associated.

Intelligence and Learning. Spence and Townsend (154) have compared two groups of ten subjects each, one with a high average score on the Thurstone Intelligence Test and the other with a low one, with respect to rate of learning a high relief finger maze. In terms of group differences the high group is clearly superior by all criteria, and the correlations between maze records and intelligence are uniformly negative within the high group, but negative only in the case of errors within the low group. The authors conclude from an analysis of the learning curves of the two groups that learning is qualitatively the same in both, the only difference being in rate. Wilson (185, 186) has continued an analysis of the data obtained from his former study (1927) of the learning of bright and dull nine- and twelve-year olds. In placing blocks in the Goddard form-board with eyes blindfolded, after the subject had two trials with vision, accuracy shows a considerable superiority for the brighter group and there is an equal percentage reduction in errors. In a game, success in which depended upon the application of a principle, the bright were again superior. The characteristics of the learning of the two groups appear qualitatively the same.

No correlation appears between intelligence rating and improvement from practice at the Müller-Lyer illusion (Crosland, Taylor and Newsom, 30). Interestingly enough, subjects of higher rating tend to overcorrect the illusion figure in the later stages of practice, while those of lower intelligence tend to undercorrect it. Knotts and Miles (92) have employed a stylus and a finger maze, of identical pattern, with both blind and sighted objects. The correlations between mental age and maze criteria are mostly negative, and some are high. The composite scores of three of the groups which learned a stylus maze for Peterson and Allison (132) correlate between 0.02 and 0.23 with Army Alpha and McGinnis (111) finds ability to learn a maze pattern not highly correlated with I.Q. With a maze situation in which insight could be used, however, the correlation with intelligence is higher. Gemelli (52) holds that intelligence is undoubtedly a factor in manual skill.

Age Differences. The paucity of studies directed specifically at the influence of age upon learning is noteworthy. There has been but one such study, that of Husband, and a few incidental results. This decline in the amount of work published upon the age factor may probably be interpreted to mean a recognition that the general out-

lines of an answer to the question of the effect of age are already available and that further work without a fresh and more detailed analysis of the problem is futile.

From a study of the maze performances of older graduate students and of college student controls, Husband (**82**) concludes tentatively that persons over thirty are inferior maze learners to those around twenty. There are indications that the difference is greater still at forty. In practice at cutaneous localization (Renshaw, **134**) children show a more rapid decrement in error than do adults. Kern (**89**) formulates an ontogenetic "law of practice" which is that the younger the learner the lower his initial accomplishment and the higher his percentage increases from practice. This is corroborated in general by Hicks'(**64**) data on target-throwing. The sighted subjects in the Knotts and Miles (**92**) study give definite inverse correlations between chronological age and both stylus and finger maze criteria, while the blind subjects give coefficients which are inverse but insignificantly small in the case of the finger maze and higher positive ones in the case of the stylus maze. Harter (**62**) reports that, in performance on three non-verbal tests, subjects who succeed are older chronologically than those who fail.

Sex Differences. The problem of sex differences in learning is still less active than is that of age differences, and presumably for the same reason. A few incidental results may be cited. In Leuba's (**104**) investigation of the influence of incentives, the boys improve somewhat more than girls under the incentives, but the boys had a smaller output under the no incentive condition. This fact is stated, not as a valid instance of sex differences *per se,* but as an illustration "of the general principle that those gain most in output with the introduction of incentives for whom the work *as such* has the least stimulating value." McGinnis (**111**), using the Young Slot Maze A, found early in learning a distinct difference in favor of the boys at each of ages three, four and five, but this difference tended to disappear as learning progressed. It is suggested that the initial advantage of the boys is a function of their better pattern-seeing ability. In that case the fact of the later disappearance of differences would indicate no real disparity between the sexes in sheer learning ability. Boys excel girls in learning skill in target-throwing (Hicks, **64**). Sanderson (**140**) reports no sex differences in maze learning, and no sex differences in original ability, in variation, or in improvement appear in Guilford's (**58**) experiment on learning to read facial expression.

The Influence of the Receptors. The old question of which sense organ is superior for learning has practically been abandoned. In its place remain the somewhat related problems of the comparative learning abilities of the blind and the seeing, and of the influence of the motor organ employed in learning.

Knotts and Miles (**92**) have employed both a stylus and a finger maze with two groups of 20 blind subjects each and with two of 20 sighted subjects each, the blind and sighted being matched in C.A. and in M.A. The blind subjects have median scores on both mazes which tend to be superior to those of the sighted. The blind show the wider range of scores. The more successful blind learners tend to be those whose blindness is due to accident after the age of five years. A greater proportion of the blind use the more efficient verbal method of learning. The work of Renshaw, Wherry and Newlin (**135**) indicates that, for cutaneous localization by either blind or seeing subjects, the optimum receptor is a matter of degree of practice.

Smith (**149**) practiced reading pointscript, using each hand alone and reading both from left to right and right to left, and using both hands in both directions. These six conditions were gone through serially and practice effects from condition to condition were controlled computatively by the use of Meyer's arc cotangent function. The conclusion is, that for this subject, extension is very slightly superior to flexion and that the left hand is definitely superior to the right when it functions as a sense organ. The hypothesis offered in explanation of this left hand superiority is that the left hand is more often employed as a sensory organ, while the primary effector organ, the right hand, is left free for direct motor activity. Merry (**113**) and Swift (**161**) have offered certain criticisms of these data and of their interpretation.[1]

Comparative Studies of Learning. In spite of the vigorous research upon animal learning which has been in progress for more than thirty years, seldom have comparisons between the learning of human and infra-human subjects been made. This lack of comparative studies is the more remarkable when one considers that most of the animal work is done avowedly for comparative purposes rather than in the interests of the construction of a psychology of the lower animals. In the absence of systematic comparisons, the "comparative" significance of the data on animals is highly inferential at best, and without such systematic work an adequate account of the evolution of learning can scarcely be approached. The only paper in which the general factors in the learning of rats and of human beings, for

example, have been contrasted is, according to Husband (**80**) that of Hicks and Carr, published in 1912. Others have made comparisons in connection with specific problems.

Husband's experimental comparison of 20 human subjects and 43 white rats on the same maze pattern is an important approach to this problem. There were certain exceptions to the comparability of the conditions under which the two groups learned: the human subjects learned a finger maze at a single sitting under fairly complete instructions, the rats learned an enclosed path through which they ran with a distribution of one trial per day. Under these conditions the human subjects averaged 16.7 trials to learn, the rats averaged 24.2. The rats were the less variable. The learning curves are similar in shape, with that of the rats always the lower. The human subjects learn the outside portions first and their errors show an alternation tendency. The rats show a tendency toward a regressive order of elimination and their errors indicate a tendency to maintain a given direction. The finger maze is a considerably easier problem for the human subjects than is the stylus maze of the same pattern. In terms of trials, the pattern used by Nyswander (**120**) presented, in high relief form, only two-thirds of the difficulty which it offered in stylus form. Employment of the easier form of human maze would weight the set-up against the rats.

A child, aged two years seven months at the beginning of the experiment, and with I.Q. 141, was put through a series of tests like those used by Köhler with chimpanzees (Brainard, **11**). The child had approximately the same difficulties with the problems as did the apes. There is the same general approach and the same appearance of emotional stress in the face of difficulty. Both are highly selective, giving the appearance of purpose.

Racial studies come, also, in this field. Lanier (**96**) has made a comparison of the performances of twelve-year-old white and negro children on the Peterson Rational Learning problem. The data are from the Peterson-Lanier study of racial differences. The Nashville whites generally excel the Nashville negroes, but the Chicago results yield no reliable differences. The New York differences are not uniform. Relatively, the negroes do their best work in terms of trials, unclassified and logical errors; the whites in terms of total time, perseverative errors, and rate of response. The most decisive white superiority is in speed.

Serial Position Effects. The data of Knotts and Miles (**92**) show a general primacy-finality superiority, but there is no regular decline

in rate of learning from these points toward the more central positions in either stylus or finger maze with either blind or sighted subjects. The important factor is rather the sequential relations of the culs-de-sac. In Husband's (80) study the serial position values, errors, for rats and for human subjects correlate 0.04. The latter yield roughly the same tendencies as were found by Knotts and Miles. The rats give very irregular serial position results, with a suggestion of a regressive order. McGinnis (111) finds a general primacy-finality effect with young subjects and two maze patterns.

Perseveration. The general problem of the nature and conditions of perseverative phenomena has possible interpretative significance for several problems in learning, as, for example, distribution of practice and retroactive inhibition. Jasper (86) defines it as "the tendency of a set of neurons, once excited, to persist in the state of excitation autonomously, showing resistance to any change in this state." It is, then, a kind of inertia of the nervous system. Perseveration could also, it seems, be defined in purely functional terms which would make no assumptions whatever about neural inertia. Jasper reviews the literature and applies a series of tests in an effort to discover whether perseveration is a functional unit participating in all behavior processes. No evidence is discovered for the hypothesis of a general group factor of perseveration. There is some evidence for a "narrow group factor of motor perseveration participating in a number of disparate motor processes which require a more or less rapid shift from one 'pattern' of response to another 'pattern' within the same general type of response. . . ." This conclusion seems, *à priori,* to make perseveration a possible interpretive concept in the case of some of the problems of motor skill. Perseverative responses in young children are numerous below two years of age and decrease sharply from that point to five years (Brian and Goodenough, 12).

Comparisons of the Difficulty of Learning Problems. Two patterns each of the stylus and finger maze construction have been compared by Nyswander (120). The finger mazes were consistently the easier, requiring from one-third to two-thirds as much learning effort as did the stylus mazes. The ratio depended on the maze and the criterion which are placed in contrast. The finger maze was likewise uniformly easier for the subjects employed by Knotts and Miles (92).

Length of Problem. A comparison of the influence of the length of Peterson Rational Learning problems upon the difficulty of learning has been made by McGeoch and Oberschelp (110). Problems of

12 and 18 letters are more difficult to learn than are 6-letter problems to a degree which is markedly disproportionate to their respective lengths. Increase in length from 12 to 18 letters is accompanied by an increase in difficulty which is more nearly proportional to the increase in length.

Maze Learning with the Time Factor Constant. Husband (**81**) has studied the learning of a high relief finger maze at a constant rate per unit of path. The general characteristics of such learning are the same as those of the control, but the control curve is the lower. The differences between the groups become more pronounced late in the learning.

Emotion and Learning. Landis' (**95**) chapter on the expression of emotion, in *The Foundations of Experimental Psychology,* is rich in evidence of the importance of the concept of learning for an adequate interpretation of the experimental work on emotion. He points out, in concluding, that "the most important line of future research is that of the nature of the relation existing between emotion and learning, in the broadest sense of the terms." Using a fifteen months old boy as his subject, Jones (**88**) has demonstrated the conditioning of a fear-avoidance response, with electric shock as the primary and a bell as the secondary stimulus, and has shown upon this CR a large number of the characteristics of CR phenomena.

Fatigue Phenomena. Glaze (**56**) has reworked the data on speed of writing the letters *ab,* from his 1928 experiment on fasting, to show the effect of practice on the decrement. Repeated performance masks the effects of fatigue, especially in the early parts of the 20-minute practice periods. Efimoff, Sarch and Krasnikowa (**40**) point out, also, how fatigue may, in a practical skill, mask practice effects. The interrelations of practice and the decrement are a complex problem and one which appears ubiquitously in experiments whose practice periods are of any considerable length. In such experiments the improvement which appears must be a remainder after the subtraction of the amount of the decrement. The difficulty of interpretation lies in our ignorance of how much of the practice effect has been cancelled and, therefore, what our measured remainder really means. In some cases the latter may be a small and in others a large proportion of the actual practice effect, and whether it is the one or the other might conceivably make a major difference in the interpretation of the effect of the experimental conditions. This is a problem which merits extended experimental attack.

Sleep and Hypnosis. Weiskotten and Ferguson (**179**) have

studied the influence of two nights of insomnia upon three learning problems; tossing balls at a target, translating from the Morse code, and mental multiplication of two-place by two-place numbers. There were three experimental subjects and two controls. Tests were run for an 18-day pre-insomnia period and for a 6-day post-insomnia period. The curves fluctuate somewhat more during the insomnia period but pick up and continue normally thereafter. Speed suffers more than accuracy. The authors think that fatigue, as such, does not affect ability, but that it lessens motivation and concentration of attention. A popular book on sleep by Laird and Muller (**94**) has some data in the appendix upon the influence of varying amounts of sleep on learning.

Conditioned finger-defense reactions are set up more readily in the hypnotic trance than in waking and may be elicited in the succeeding waking state, although the subject can recall nothing of the conditioning process (H. D. Scott, **143**). In his more general criticism of Hull's projects in hypnosis Stein (**157**) has objected to Scott's conclusion on the ground that the index of conditioning used was inadequate. Hull's (**74, 75**) outline of problems in the field of hypnosis contains several which involve significantly the acquisition of acts of skill.

Individual Differences in Motor Skills. While the diagnosis of individual differences in skill lies outside the scope of this review, many of the results on the problem have a bearing, interpretively, on the issues of acquisition. Seashore's (**147**) paper contains a great deal of valuable information of this sort.

The Problem of Types. The search for type classifications still goes on in some quarters. From data on 30 subjects Rubinstein (**139**) differentiates the tempo-plastic and the tempo-inert types. The former shows an increased time with advancing complexity of performance, while the frequency of his errors remains the same; the latter shows an unchanged time but an increase in errors. From a study of several tests Robert (**137**) describes four types from the standpoint of perfectibility: the superior, inferior, average and mixed. A subject could hardly escape one of these.

Initial Status and Gain. Kern (**89**) summarizes a variety of data in a differential law of practice that low initial achievement is associated with high absolute gains, and points out its limitations. Kern also takes up the question of when stability of relative position is reached in learning. He finds, from the use of six tests, that it is never reached in the early trials, and that the point is very difficult

to ascertain from the usual learning experiments on account of the operation of at least the four factors of lack of uniform motivation, variation in adaptation to the experimental situation, differences in susceptibility to fatigue, and varying physiological limits. It varies from test to test, but once the level of stability has been reached, it tends to remain constant. The correlation between initial and best trials and between the means of the first five and of the last five trials of a maze are very low save for the four-year-old group in McGinnis' (**111**) experiment. All three age groups yield high inverse correlations between initial trials and amounts gained. In Gemelli's (**52**) tests of a practical nature, the correlations between tests made after six months and those made earlier increased as the time from the beginning of the experiment increased. Guilford (**58**) reports a negative correlation between initial ability to judge faces and improvement in that ability. This is explicable, he thinks, in terms of differences in attitude. Initial ability in the perception of the Müller-Lyer illusion figure is only slightly prognostic of final ability (Crosland, Taylor, and Newsom, **30**). Sollier and Drabs (**152**, **153**) believe, however, that an index of perfectibility can be established by means of which final status may be predicted from early performance. Cureton (**31**) has proposed an improvement upon the Thomson formula for the correlation between initial status and gain.

Reliability and the Intercorrelation of Criteria. Nyswander (**120**) has compared the reliabilities of two stylus mazes and two finger mazes, and finds no clear superiority for either kind. She concludes that maze reliability depends partly on factors other than mode of construction. Pattern and number of blinds may be effective factors. In a very difficult maze, learning in the early trials is such an unintegrated process that individuals are not differentiated with any certainty. In a too-simple maze, likewise, no differentiation will occur. She points out that many of the correlations between intelligence and motor learning have suffered from a failure to determine the reliabilities involved. The finger maze is, she thinks, an important instrument and one can be constructed to give more reliable scores than a stylus maze of the same pattern. In Husband's (**80**) study the reliabilities of the errors on odd and even trials are 0.95 for human subjects and 0.88 for rats; on first and second halves, 0.95 and 0.86, respectively. The reliability coefficients for maze scores, sums of time and errors on odd and even trials, reported by McGinnis (**111**) are between 0.81 and 0.98, in one case, and considerably lower in another. The increase in reliability resulting from visual exposure prior to

each learning trial has already been mentioned (Peterson and Allison, **132**). The increase from 0.48 with no visual exposure to 0.87 with 10- and 20-second exposures is considerable.

The intercorrelations of the trial, error and time scores in the Knotts and Miles (**92**) experiment range from 0.88 to 0.98 for blind and sighted subjects on finger and stylus mazes. Spence and Townsend (**154**), correlating time and errors, odd and even trials, obtain coefficients all of which are above 0.90, and the maze criteria of Peterson and Allison (**132**) intercorrelate between 0.59 and 0.84.

Transfer. The general problem of transfer, both positive and negative, is perhaps the most pervasive and far-reaching of the problems of learning. As Hunter (**78**) concisely puts it in the opening sentence of his section on transfer in *The Foundations of Experimental Psychology:* "No stimulus-response coördination can be acquired uninfluenced by the action system already possessed by the subject." The functional relations of already organized systems to new systems in the process of organization are limitless and, in spite of the large number of accumulated titles on the subject of transfer, very imperfectly studied. To reduce these relations to experimental and logical order is one of the magnificent problems of the future.

Yum's (**191**) test of the law of assimilation is a basic experiment upon transfer. He raises the question: "To what extent will a motor response that has been contiguously associated with a given perceptual stimulus be aroused by a similar stimulus with which it has been associated, and to what extent is the likelihood of arousal a function of the degree and kind of similarity involved?" As material he used double syllables paired with words and introduced changed letters in the stimulus syllables at recall; and paired meaningful words, using in recall a control and words having two degrees of ranked similarity to the original stimulus words. All alterations of the stimulus reduced the amount recalled, and alteration of the first letter of either syllable, in the first experiment, had the most effect. With the paired words it was found that "a response associated with a stimulus word tends to be aroused by a novel word having a similar meaning and that the likelihood of arousal varied directly with the degree of similarity." This was verified by another experiment using visual patterns.

The work on intention in motor learning, by Sanderson (**140**), is related to the problem of intent to learn in experiments on both skill and memorization, as well as to transfer. His subjects learned a complex maze with no culs-de-sac, and a printed sheet on which the

numbers from 1 to 40, when connected by a continuous line, would reproduce the path of the maze. Two groups were given no instructions regarding pattern but were told to work rapidly; and two were told to learn for reproduction of pattern. When both speed and pattern groups shifted from number sheet to maze, to be learned for speed, the pattern intention proved superior. Less transference occurred from maze to number sheet, with no superiority for the pattern intention. Dorsey and Hopkins (**36**) have measured the transfer effect of methods of study, knowledge of Latin, and skill in manipulating special elements in descriptive geometry, when the subjects are instructed to use the knowledge gained on the original material in handling the new. Equated control groups were not given the special instructions. The amounts of transfer vary with the method of measurement and with the material, but positive advantage for the experimental group in fairly substantial amounts is the rule. An extensive investigation has been conducted by Beeby (**5**) into the organization of simultaneous constituents in a continuous tracing skill. Both positive transfer and interference appear in the transfer from single- to double-handed practice, and the important conclusion is drawn that with continued practice initial positive transfer may shift to negative transfer. Further, the presence or absence of vision is a factor. With vision excluded there may be interference; with vision permitted there may be positive transfer.

Considerable amounts of transfer occur from maze pattern to maze pattern in spite of the fact that the patterns were designed to produce a maximum of interference (McGinnis, **111**). The initial trial of a new pattern might show some interference, but this rapidly disappeared. When subjects learn Peterson Rational Learning problems 6, 12 and 18 letters in length, McGeoch and Oberschelp (**110**) find an increase in the ease of learning the 6- and 12-letter problems as a result of practice at one another and at the 18-letter problem. Transfer to the 18-letter problem does not occur consistently. A possible explanation of the latter fact is that transfer is a function of the amount of the preceding practice measured in actual learning effort. On this assumption one would expect less transfer from the shorter to the longer problems.

The techniques of grammar, such as learning of rules, definitions and principles, and the analysis of grammatical constructions have, according to Symonds (**162**) much more effect upon correct English usage than does repetition of the correct forms. In Leonard's (**102**) work practice exercises, such as proof-reading, error correction and

dictation, showed a clear positive transfer to punctuation and capitalization in written compositions. Overman (122) finds useful amounts of transfer from instruction in two-place addition to related two- and three-place addition and subtraction. Both positive and negative transfer in spelling are found by Archer (2) and the unit of transfer is sometimes as small as a single syllable. From work on practical school subjects Anderson (1) concludes that the amount of a practice effect is a function of the number of similar factors in the situations concerned. Simpson (148) reports that special training in evaluating, outlining, and summarizing historical materials aids ability to organize. Nichols (119) concludes that special training in close observation and comparison will transfer to other activities.

In his presidential address to Section J of the British Association, Pear (126) divides the problem of transfer into two parts: (A) that resulting merely from exercise of a particular function, and (B) that resulting from an extension of attitudes, ideals, methods and the like. He thinks that transfer of the first kind is rare and that transfer of the second kind definitely occurs. The same general view has been set forth, also, by Pear, Langdon and Yates (130). In a general discussion of the problem James (85) states the opinion that transfer should be sought in long training tests, not in the usual short-time tests. Transfer may not be manifest in immediate efficiency changes and may be masked at first by the efficiency loss due to readjustment and interference. Qualitative as well as quantitative changes should be investigated. Holzinger (69) corrects the probable error of a difference formula employed in Gates'(1924) paper on methods of measuring transfer.

Two sharply contrasting interpretations of the facts of transfer are to be found in the writings of Hunter (78) and of Wheeler (181). The former closes his discussion of the experimental work on transfer with the statement: "We are still far from a satisfactory explanation of the facts of transfer. At present the theory of identical elements seems most adequate, and yet it is all but impossible to bring the theory to a rigorous experimental test." An opposing interpretation is expressed in both of Wheeler's books (181, 182). In the latter he writes: "Transfer is not dependent upon common elements because there are no elements from the standpoint of organismic psychology. Rather, it is a case falling under the third organismic principle, namely, that the whole governs the activities of its parts." Clearly, an adequate theory waits on crucial experiments.

Apparatus and Method. A valuable chronological outline of maze

studies using human subjects, with a description of the maze, the number and kind of subjects and the topic studied has been published by McGinnis (111). The following pieces of new apparatus have been described within the two-year period: a mechanical model which will show the major phenomena of the conditioned response—push buttons represent receptors, a flashlight bulb is the responding system, copper wires are the nerves, and synapses are represented by mercury toluene thermoregulators, Baernstein and Hull (3); a mechanical device which illustrates the setting up of a conditioned response, Walton (174); a symmetrical linear maze pattern designed for analysis of serial learning, Warden (175); a stylus maze with alleys of triangular cross-section to be run with a ball-pointed stylus, Schlosberg and Carmichael (141); a maze composed of a discrete path formed by escutcheon pins inserted in perforations in a tin sheet and readily alterable, Chou (25); a recording pencil maze, Franz and Watson (45); a form of stylus maze which is mechanically proof against retracing, T. C. Scott (145); a tridimensional maze consisting of a system of brass pipes through which a bearing is to be run without permitting it to drop out, Boder (7); a multiple groove rolling board consisting of 13 rows of blocks an inch square and 13 columns of grooves, the problem being to roll a bearing over any prescribed path by tilting the board in one's hands, Chou (24); four pieces of apparatus for serial reactions, usable for studies of habit formation, Dashiell (33); a Yerkes multiple choice apparatus which can be made in quantity at a reasonable cost, Zener and Wever (192); a new puzzle box which can be used as a learning problem, Brümmer (17); a simple one-handed motor task, Miles (114). New methods of treating experimental data have been published by Holsopple and Feldstein (67) and by Luh and Liang (106).

II. Learning—General and Theoretical

The work reviewed in the preceding section has been concerned with issues which are primarily factual and while theoretical discussions have been involved they have been directed mainly at special problems. In one sense, of course, all facts of importance point beyond themselves to larger theoretical structures and significant theories imply future experimental programs. In that sense there can be no clean-cut line between experiment and theoretical interpretation. For the purposes of this review, however, a classificatory line can be drawn and the present section will consider, as well as general

summaries and purely theoretical papers, experimental work which has theoretical implications of some systematic generality.

Definitions of Learning. The problem of what the concept of learning may validly include is one of major importance. Two of the most important general treatments of the field, by seasoned experimentalists, evidence a clear recognition of this problem and offer modes of solution. Lashley (**98**) surveys the phenomena, from hysteresis to logical memory, which have been included in the category of learning, and concludes: "With this variety of description and interpretation we are justified in raising the question whether the concept of learning or of memory embraces a unitary process which can be studied as a single problem, or whether it may not instead cover a great variety of phenomena having no common organic basis. The question cannot be answered until the physiological mechanisms of a variety of habits and memories have been worked out." Evidence may be sought in the development of and in the result of injuries to the nervous system, in an analysis of the sensory and motor components of habit, and in the conditions under which "learning" occurs. Hunter (**78**), likewise, notes the difficulty of determining what constitutes learning and formulates this constructive definition: "In general, however, we may say that learning is taking place wherever behavior shows a *progressive change,* or *trend,* with a repetition of the same stimulating situation and where the change cannot be accounted for on the basis of fatigue or of receptor and effector changes."

To Thorndike (**165**) "human learning consists of changes in the nature and behavior of human beings." Behavior includes thoughts and feelings as well as movements, and the real changes are in S-R connections. The latter are "tendencies for a given situation to evoke certain responses rather than others," and their strengths are expressible in degrees of probability that a given S will elicit a particular R. Since new responses always come in connection with something, learning is really a matter of changing the strengths of connections. Humphrey (**77**) points out that learning is a four-dimensional adjustment, and that the time-coördinate must be included in the total situation. Learning is defined by Wheeler (**181**) as "behavior in terms of which the individual extends his insight into a given situation and increases the complexity of his actions with respect to a certain goal."

The term "learning," at least as it is used by the educational psychologists, has, as Corey's (**26**) analysis is designed to show, no

settled and standard meaning. It is used in the sense of growth, as synonymous with memorizing, to represent any more or less permanent change in behavior not due to maturation, as synonymous with education or approaching a goal, and as connection-forming.

Definitions of Skill. In his presidential address to Section J of the British Association, Pear (**126**) repeats his well-known definition of skill as "an integration of well-adjusted performances." It is dependent, but not entirely, on habit. As distinct from habit, it "involves the ability to be aware of and to correct, imperfect or faulty adjustment." The same ideas are summarized in another paper (**125**), and in a third (**127**) he differentiates skill from intellectual, moral and character traits. He points out (**128**), in an analysis of "some subtler skills," that there is more implied in the concept of skill than is usually recognized. These skills differ from the less subtle only in degree, but their components are more elusive. Under the subtler skills he lists, *e.g.*, verbal habits, mathematical skill, literary composition, preparing speeches, music and even the visual image. This view carries the concept of skill into terrains where experimental analysis has as yet seldom gone, at least under the name of skill. It is a view which opens a large field and one which deserves systematic handling. It may at least be ventured here that an adequate set of interpretive concepts ought to reach from mazes to writing and music. Pear (**129**) has suggested the term "euphasia" for the "ability for deliberate, adequate, verbal expression," which is a special type of skill but important enough to deserve a special name. White (**183**) objects that euphasia is physiological, unnecessary and inadequate.

Gemelli (**53**) contributes an extensive and interesting description of four categories of skill and a phenomenological analysis of manual skill, with an outline of the elements thereof. An analysis of manual skills has also been made by Schorn (**142**). Her work, as is Gemelli's, is typical of a large amount of psychotechnically directed writing. The complex concept of skill must, she thinks, be subdivided into skill in the sense of manual readiness, of quick, apt movements, and of cautious, patient operations. Wallon (**173**) also, has made an examination of the factors upon which manual skill depends.

Definitions of Habit. It is Bruce's (**15**) thesis that in educational psychology habit is often confusingly described as both fixed and flexible. To him the unique characteristic of habit is the persistence of the essential features of an activity in spite of external changes. The stability of habit depends on the purpose of the individual, the flexibility is the means of accomplishment. In a second paper,

Bruce (16) has examined the relation between habit and intelligence and has particularly attacked the view that habits are unanalytic while intelligence is analytic. He sets out to show (A) that habitual skill requires analysis, (B) which is the same in kind as that of intelligence, and (C) that intelligent conceptions may be, as may habits, so compactly organized as to defy verbal description. Tuttle (167) points out that there have been two different elements in the definitions of habit since the time of James: (A) habit as fixed behavior, and (B) habit as a preferred way of responding. In the latter case, where an element of value enters, we have an attitude, not a habit. The ambiguities noted by Corey (26) in the definitions of learning appear also in the treatments of habit. It is used by some as equivalent to learning, by others as the tendency of functions to become automatized.

Three papers have attacked habit from a philosophical approach. Chevalier (22) uses the concept of habit as a means of reconciliation between metaphysics and science. The term is used to mean an internal disposition produced by custom. In its passive form it means the acquisition of a mode of existence, and in its active form an actualization of aptitude. His subsequent discussion of the problems of habit is at the level of philosophy rather than of psychology. Lefevre (101) compares habit and "habitus" and concludes that the latter is an intellectual matter, a modality of being, while habit is a function of the nervous system. Dehove (34), likewise, presents a philosophical treatment of habit.

Books and General Summaries. Murphy's *Historical Introduction to Modern Psychology* (116) contains a good chapter on the early experiments upon the acquisition of skill. The work of Bryan and Harter, Book, Thorndike and a few of the later investigators is described. It is interesting that the term "skill" does not appear in the index of Boring's *History of Experimental Psychology* (9) and that, in the body of the volume, learning, as distinct from memorizing, receives its chief attention in connection with the rise of animal psychology. This omission has been aptly criticized by Robinson (138), who writes: "The reviewer has the feeling that the pioneer studies of Bryan and Harter, and of Book on the acquisition of skill were of substantial importance even for the development of a generalized human mind."

Holt's *Animal Drive and the Learning Process* (68) presents "the outline of a non-faculty psychology in terms wholly of physical and physiological processes." It is written to build up a theory of

mind as produced by the bodily machinery rather than to solve directly, by frontal attack, the problems of learning. Considering the pervasiveness of the concept of learning, this more general method of approach may be preferable. Certain of Holt's special theories will be reviewed later. The special chapters by Lashley and by Hunter have been mentioned already. Each is a major contribution to the bringing of order into a complex and difficult experimental province. In a volume entitled *The Growth of Ability,* Filter and Held (**44**) have discussed certain of the problems of skill.

In Garrett's *Great Experiments in Psychology* (**50**) there are several chapters on learning and related problems. The work of Pavlov on the conditioned response, Thorndike's experiments on animals and the laws of learning, the experiments of Thorndike and Woodworth on transfer, and the Gestalt experiments on learning are clearly described. Wheeler's *Readings in Psychology* (**182**) reproduces three papers of importance for skill, viz., Cason's criticisms of the laws of exercise and effect, a note by Cutsforth on insight in the learning of Helen Keller, and Bray's paper on transfer. Wheeler's editorial notes before each reading are extremely valuable comments on the configurational point of view as applied to learning. Powers'(**133**) review of the literature on language learning is an important source of information in that field.

Rexroad's (**136**) text book has a consistent conditioned response interpretation of learning. It is particularly characterized by the thoroughness and completeness with which the conditioned response is applied diagrammatically to the problems of learning. Warren and Carmichael (**176**) give a compact account of learning based on modified associationistic principles. Three of the chapters of Wheeler's (**181**) text are devoted to the most thoroughgoing configurational account of learning which has yet been published. Muse (**117**) reviews for college students the factors in economical learning with a view to improving techniques of study, and Bloor (**6**) has written a book on learning from the pedagogical point of view.

The Law of Frequency. The most active experimental problem which points primarily to a general theoretical interpretation of learning has, during the last two years, been that of the influence of frequency. The principle, so long tacitly accepted as established beyond doubt, that repetition in itself promotes learning, has been sharply challenged. Thorndike's (**165**) work, reported in his Messenger Lectures at Cornell University, is the most extensive. In one case the subject was instructed to draw a four-inch line with one quick

shove of the pencil. In another, given the beginnings of words, such as *ad, bi,* and the like, the subject was to complete the words, making 240 completions daily for 14 days, with certain beginnings occurring 28 times. The question which these and similar experiments were designed to answer was whether mere repetition of a situation will, in and of itself, produce learning. The answer is unequivocally that it will not. In the first experiment mentioned, the lines drawn at the end of the practice were not appreciably different from those drawn at the beginning. In the second, there is change but not in the direction of the most frequent situations. A coördinate question is whether frequency, not of situation but of connection, causes learning. The general plan of the experimental program was to present the subject with long series of pairs, as names and numbers, in which certain pairs recur often, and to instruct him to listen to them without effort to remember and without rehearsal; or he was asked to write them under the belief that the purpose was to study fatigue or some non-learning problem. Sentences, also, were read to the subjects, who were then asked questions thereon, some of which tested connections from the end of one sentence to the beginning of the next and some of which called for connections within the sentence. The outcome was that frequency of mere temporal contiguity is powerless, but that when the connections were intrinsic to the material, *i.e.,* it "belonged" together, learning took place. This introduces a new principle, that of *belonging.* Repetition plus belonging and acceptability will yield learning, but not readily. Another principle is needed, which Thorndike finds in the after-effect of the connections. This will be reviewed presently.

This principle of belonging, which would be strengthened by further systematic elaboration, is, to some extent at least, designed to meet Hunter's (**78**) comments upon Thorndike's earlier published (1927) experiment on frequency. Hunter writes: "The phenomenon of incidental memory has long familiarized us with the impotency of mere frequency of stimulation and response in producing learning, but it is not clear (although it may be true) that incidental memory needs only the introduction of an effect factor in order to have its status changed. In order to modify behavior significantly a stimulus-response must apparently dominate the organism at the time when it is repeated, dominate in some such manner as the old associationists meant when they said that things must be ***experienced together*** in order to be associated." The experiments on conditioned

reflexes, he urges, are another reason for believing in the efficacy of frequency.

In his theoretical interpretation of the experimental work upon the influence of guidance in learning, Carr (19) raises the question whether frequency is a universal law. Is an act always strengthened by practice or only under certain circumstances? This leads at once to a consideration of effect, to Carr's treatment of which we shall return under the heading of that law. The law of exercise, he concludes, "explains the *rate of the selective and eliminative process*," but to do this the usual statement of it must be modified to read: "The *rate* at which acts are selected or eliminated is a function of the *frequency* with which they are exercised. The more frequently an *unsuccessful* act is exercised the quicker it is *eliminated* so far as that situation is concerned; the more frequently a *successful* act is exercised the sooner it is *selected or fixated* so far as that situation is concerned." Practice may fixate or it may, as Dunlap (1928) has contended, disrupt. It is, moreover, "improper to say that is it the *practice per se* that fixates or eliminates acts. It is the stimulations that result from their performance in a given situation that are responsible for their selection and elimination," while rate is in part a function of frequency.

Dunlap (39) comes to the defense of his theory of the influence of repetition which is expressed principally in the beta hypothesis "that response, in itself, has no effect on the probability of the same stimulus pattern producing the same response. . . ." His deduction from the beta postulate that repetition may be employed in breaking habits as well as in forming them is supported by work on thumb-sucking and enuresis in children, on confirmed nailbiters of college age, on sexual perversions in adolescents and adults, and on stammering. Experimental work on the theory is now the important thing, but the data already available lead him to this significant statement: "We might as well face the possibility that we shall eventually be forced to the conclusion that no habit formation is possible without ideational activity, or thinking; a conclusion to which the so-called 'conditioned reflex' experiments, in my limited understanding of them, already point." Wakeham (172) reports a case in which Dunlap's postulate was tested upon certain errors in typewriting by an unusually intelligent subject. Practice upon repetition of the error greatly reduced that particular one but developed an error of a converse kind. Apparently, it is concluded, Dunlap's theory may cut two ways. Improvements upon this technique, designed to avoid the

production of the new error, have been suggested by Dunlap (**39**), who states that technique is all important in testing the beta postulate. Nathanson (**118**) has described some cases of habit modification on a theory developed by Twitmyer. It is held that to remove an habitual act it must be lifted back to a conscious level. Working upon this theory, individuals with undesirable habits of which they wished to rid themselves were asked to keep a list of each occurrence of the undesirable response. The technique was successful with a variety of cases. It will be noted that this procedure has much in common with the application of the beta postulate by Dunlap.

A major attack of a different kind appears in experiments on the general problem of the readier resolution of physiological and psychological sequences. Freiberg, Dallenbach and Thorndike (**48**) have tested the recurring statement that the frequent occurrence of a series of physiological or psychological events will tend to produce the omission of one or more of the intermediate events. They have examined the mistakes made by the compositors in setting 1,068 galleys of the *American Journal of Psychology*. The work of a compositor is taken as typical of the kind of series implied in the statement tested. If frequency causes omission of terms, the mistakes should show that omissions are more frequent than additions, occur oftener in intermediate than in final letters, and are more common in words often set than in those rarely set. The tabulated results do not satisfy any of these expectations and the authors conclude that the facts do not support the generalization tested. This negative conclusion has been questioned by Hollingworth (**65**) on the following grounds: (A) Learning to spell involves deliberate training against omission of intermediate terms, and hand compositors are chosen on the basis of such correction. (B) The law is ordinarily given as one of learning, not as a law of the operation of habits, and studies of learning to spell have shown that the law holds there. (C) The spelling situation is not a case which validly tests the law. Had the experiment yielded positive results, they would be important; negative evidence of the kind found is meaningless. Against this criticism Dallenbach (**32**) has urged the following points: (A) why should training counteract the dropping out of intermediate items more than it does the making of other errors? Since errors occur, the law, if valid, should operate. The compositor, moreover, is taught to follow copy, not to spell. Words are set as series of events and if he learns to spell, it is a byproduct. (B) Not all will agree that the law is one of learning only. (C) Hollingworth's third point contradicts his own

argument that the law is operative in learning to spell. Mendenhall (**112**) has analyzed letter by letter large numbers of words as spelled by pupils in Grades III and IV. In words of any length the greatest number of errors occurs in letters at the center or just to the right of the center of the word. In general, letters occurring most frequently are most often in error. According to Hull (**73**), in a sequence of items or events, the intra-serial competition could override simple chaining under frequency and enable the final act to be elicited by the initial one.

It is often assumed, Guthrie (**59**) writes, that mere repetition increases the attachment of the conditioning stimulus to its response. Actually, however, "improvement demands more *detachment* of stimuli *from* responses than *attachment* of stimuli *to* responses." At the end of the training the individual is doing something quite different from what he was doing initially. The law of exercise may be a mistaken one and conditioning, at least of an elementary sort, may be an all-or-none matter. Repetitions may enlist increasing numbers of stimuli as determiners, rather than act directly on connections. Lashley (**98**), likewise, questions the universality of frequency as a principle of learning. Much learning seems to be of the all-or-none character and is more a change in the acts performed than improvement of specific acts. Moreover, the facts of the experimental extinction of conditioned responses, and of "going stale" from too much repetition, show that frequency may also have a negative effect. In the learning of localizing movements Ewert (**42**) notes that the learning consists of a gradual decrease in the number of errors rather than in the extent thereof. This indicates an all-or-none elimination. Thorndike (**165**), on the other hand, questions the presence of the all-or-none law in learning on two grounds: (A) Any connection probably involves the simultaneous action of many neurones. (B) Experimentally "we find gradualness as a fact in the simplest and most elementary connections we can devise."

In explaining the growth of habits, Watson (**178**) repeats his frequency-recency explanation. The work of Peterson and of others, which has usually been interpreted adversely to the doctrine is, Watson asserts, not crucial. In his theoretical analysis of the influence of exercise and effect Carr (**19**) has pointed out that frequency and recency are "but *two* of the *remote* effects or resultants of success and failure," and that, moreover, Watson tacitly assumes the law of effect in his formulations. The doctrine that in the long run the right response will have frequency in its favor and hence be selected,

Thorndike (**165**) asserts, to be unsound factually. Often an initially strong connection which has occurred frequently is displaced by one initially weak but having favorable consequences.

Extinction and negative adaptation constitute, as Ogden (**121**) points out (and as other writers previously reviewed have noted), a reversal of the usually found results of exercise. Humphrey (**76**) believes that the two phenomena are mutually involved and that the conventional view that two different mechanisms are present is not justified. Extinction is not due solely to repetition. At the same time food is not being given and this alteration of the conditions is also an operative factor. Winsor (**188**) likewise argues that extinction and negative adaptation are the same kind of process, in contradistinction to Woodworth's (1929) dual classification. Both are, as Woodworth states, forms of learning, but Winsor considers the differences between them less consequential than Woodworth believes them to be. In another paper Winsor (**187**) cites a case in which a positive stimulus is rapidly transformed into an inhibitory one by repetition. He proposes this law: " When an S-R activity occurs repeatedly just prior to or concurrent with an excitatory or positive S-R activity of greater potential value, the S of the former activity comes to assume some of the stimulation properties of the S of greater potential." Zipf's (**193**) principle of relative frequency as a determinant of phonetic change is interesting in this connection.

A few experiments have yielded incidental results which agree with the main tendency of the attack upon the efficacy of frequency. Mere repetition of correct grammatical forms produces, Symonds (**162**) finds, insignificant gains. Hamilton (**60**) obtained a slight improvement in accuracy of discrimination from repetition alone, and in connection of chords with reaction keys Freeman (**47**) reports that mere repetition is very uneconomical. The one apparent exception to the general tendency lies in the fact that Crosland, Taylor and Newsom (**30**) find a definite practice effect without overtly given knowledge of results after repeated trials with the Müller-Lyer illusion figure.

Refractory Phase. In an important paper Telford (**164**) inquires whether voluntary responses produce effects which serve as a barrier against repetition. Experiments are performed using serial reaction times to auditory stimuli given at intervals of 0.5, 1, 2 and 4 seconds; judgments upon pairs of lines with varying intervals between the pairs; and the writing of numbers in response to nonsense syllables heard at different rates. The results indicate that immediately after

response to the auditory stimulus "there is a period of intrinsic unreadiness for response as shown by lengthened reaction times to stimuli applied during this interval. This period is comparable to the refractory phase of more elementary systems." The 0.5 second interval is the least favorable for rapid response. The other experiments yield comparable data. These findings have many implications for learning. The discovery of a barrier against immediate repetition is significant enough in itself, but it may be highly useful, as well, in promoting understanding of problems of all sorts in which the length of interval between responses is a factor. It also sets at least a sharply limiting boundary to recency. The appearance of practice effects most clearly at the intervals least favorable to response is significant for the theory that learning may, among other things, involve a change in the refractory period of the systems involved.

The Law of Effect. The experimental demonstration of the lack of universality of frequency *per se* leaves a clearer field for the law of effect, which is invoked by Carr (19) and by Thorndike (165). To Carr the laws of exercise and of effect are supplemental laws in that each explains a different feature of learning. Exercise accounts for the rate of learning; effect explains why particular acts are fixated and others eliminated. The latter law is stated thus: "The various acts that are elicited by a problematical situation are either selected or eliminated in virtue of their success or failure in meeting that situation, or, in other words, in virtue of the diverse character of their resultants." The "demonstrably contingent correlation of two independently observable phenomena, viz., success and selection," must be accepted as fact regardless of difficulties of explanation. It should be noted that the criticisms brought against many of the statements of the law of effect do not apply in this case. The effect of an act is the stimulation to which it leads set in the perspective of the situation which led up to the act.

The interpretations just cited are the outgrowth of a summary of work upon guidance in maze learning and apply to the relatively complex integration which the maze habit represents. Thorndike's (165) work with simple connections has led him to comparable conclusions. When subjects are placed in situations which may lead to varied responses, and when the word "Right" follows as the consequence of one response and the word "Wrong" as the consequence of another, the rewarded connection gains markedly as compared to others of equal initial strength. The weight of evidence in favor of the efficacy of this knowledge-of-result type of consequence is great

and leads Thorndike to style it "as sure as the fact of learning itself." No subjective significance attaches to this interpretation. Satisfiers and annoyers are states of affairs which produce positive or negative responses. When the consequence "Wrong" is compared with the consequence "Right," the results lead to the conclusion that annoyers do not act on learning by weakening the connections they follow, much as they may lead to a displacement thereof by others. Satisfiers seem to act more directly. Thorndike speculates regarding the nature of the action of effect and concludes that perhaps the most likely explanation is one in terms of neurone behavior.*

The action of the effect Hollingworth (**66**) terms "the outstanding mystery in the psychology of learning." A careful distinction must be made between effect and affect in treatments of the law. The older statements, and some of the newer, handle it as a law of affect. More recent work has dealt with it in terms of results, of effects. To Hollingworth, irritants, disturbances, motives incite action and the satisfaction of these motives consists "not in some added positive fact, but merely in the *removal* of the motive." The fact is that "irritants become linked to the activities which eliminate them." The occurrence of satisfyingness is but an incident. The irritant becomes linked to the act which removes it because that act terminates the series, completes a segment. The one fundamental generalization is: "Learning occurs in so far as partial antecedents become effective in evoking the terminal act." Effect is operative, thus, affect is not.

Any adequate picture of Holt's (**68**) theoretical approach to learning can be gained only by a reading of the entire volume. Only a few points, isolated from their context, can be touched upon here. In essential agreement with the authors already cited upon the factual character of the influence of the results of action, he writes that "it must be that the factor which is responsible for the learning is that which *physiologically differentiates* a 'successful' movement; and this, I think, is the sudden *release from inhibition* of the postures and reflexes which pertain to food adience, and it follows at once after the successful random movement." In support of this interpretation stands a considerable structure of physiological theory, which accounts for the influence of adient responses, those which give more of the stimulus eliciting the response, and of abient responses, those which give less of the stimulus.

* This and other views of Thorndike are considered more in detail in a review of *Human Learning* published in this number of the PSYCHOLOGICAL BULLETIN.

Stephens (**158**) has constructed a mechanism, composed of magnets and the necessary adjuncts, which illustrates the law of effect. Varying responses can be explained, he holds, only "by the conception of an increase in the relative resistance of all connections not achieving a certain effect." This will also account for the constancy of the response which does achieve this effect. This view is synthesized in the apparatus mentioned which, he admits, does not prove the theory but is "an argument in its favor." According to Stephens (**159**), also, it is the "total stimulating conditions produced by the combination of response and the external stimuli which must be regarded as the effective selective agent." His view resembles somewhat Troland's (1928) theory of retroflex action. Effect is the factor, Winsor (**187**) reports, which determines whether the reaction developed is excitatory or inhibitory. The fact, noted by Hicks (**64**), that the mean scores immediately preceding or following a successful target-throw were higher than the averages for other throws bears on this problem. In view of the current statements of the law of effect in terms of consequences and not of affect, the work on the influence of knowledge of progress and of results is strictly relevant. It has, interestingly enough, not been customary to raise theoretical issues in connection with the latter problem, nor has it been usual to inquire how knowledge of progress operates to facilitate learning. Contemporary treatments of the law of effect brings this question clearly to the foreground.

The major dissenting opinion from the factual efficacy of effect is that of Wheeler (**181**) who believes it to have been disproved by several experiments. He cites Snoddy's statement that more progress was made in star-tracing on the sides which gave the subject the least satisfaction. Many unsuccessful and superfluous acts, moreover, develop in complicated learning; and the physiological mechanism of the action of such a law is virtually impossible to envisage. Wheeler is primarily attacking a law of affect, however, and his own general point of view has room for the acceptance of the facts upon non-affective consequences.

The Influence of Maturation. More clearly than ever before the place of maturation, whether ~~as coöperant factor~~ or as supersedant agent, has been urged by students of learning. On the interpretation of stair-climbing and related experiments performed by the method of co-twin control, Gesell (**54**), Gesell and Thompson (**55**), and Strayer (**160**) rely heavily upon maturation defined as "those phases and products of growth which are wholly or chiefly due to innate and

endogenous factors." "The climbing performance of Twin C at 55 was far superior to the climbing performance of Twin T at 52 weeks, even though Twin T had been trained 7 weeks earlier and 3 times longer. The maturity advantage of three weeks of age must account for this superiority." Granting the hazard of generalization, Gesell and Thompson write, further: "There is no conclusive evidence that practice and exercise even hasten the actual appearance of types of reaction like climbing and tower building." Maturation processes, Gesell thinks, determine the form and sequence of infant behavior pattern, and the infant is secure against extreme conditioning of whatever kind. Strayer has studied the acquisition of vocabulary by these twins, beginning in one case at 84 weeks of age when both were near the threshold of speech acquisition. Twin T was given intensive training for 5 weeks while Twin C was isolated from speech influences. Twin C was then given the same course of training for four weeks beginning at the age of 89 weeks. In spite of certain extrinsic handicaps, Twin C "began to acquire words earlier in the training period, and had on each comparable day of training a vocabulary greater than that of her twin." After 28 days of training C was equal if not superior to T. The results are interpreted as due to the greater maturity of C at the time her training began. Watson (178) criticizes the conclusions of Gesell and his students on the ground that their use of the concept of maturation is not clear and that the experimental set-ups used do not validly demonstrate the influence of growth factors. Both C and T should have been used as controls in different experiments.

According to Wheeler (181) a distinction should be made in discussions of frequency between repetition of response and repetition of stimulation. The former is held to be the causal agent in the usual law of exercise. The latter "is unquestionably a condition of learning, not because it leads to the use of the same nerve patterns over and over again but because it induces maturation when the stimulation is properly timed. Hence the essential thing in learning is not the fixation of 'pathways'; it is the differential use of more 'pathways,' practically the opposite of fixation." The evidence for the importance of maturation in learning "appeared first, in the fact that both animals and human beings were able to meet *new* situations of certain degrees of difficulty the first time they tried. Second, when the organism was confronted with the same environmental situation repeatedly there was a constant and progressive change in its behavior toward a more complex, differentiated and at the same time,

organized response." The conclusion is that maturation makes these facts possible. "The logic of the theory is not altogether different from the older conception that a 'setting process' took place in the brain after every sense impression, but it substitutes the notion of growth or progressive change for the idea of traces." In criticism of this view Lanier (**97**) has urged that maturation is tautologous, as used, and is merely a synonym for learning. Holt (**68**) comments, on the general problem, that "too much of organic structure has been attributed to 'growth,' as an unfolding from within, and too little to 'learning'; that is, to the effect of the environment."

Marquis (**108**) differentiates learning from maturation by the fact that learning is a modification of pattern in response to specific present stimuli in the external environment, while maturation is a modification in response to stimuli from the intra- and inter-cellular environments which, at the moment, are free from external influences." Structural maturation, Hicks (**64**) believes, is an important factor in target-throwing by young children.

Insight versus Trial-and-Error. Some of the experimental and many of the theoretical papers already reviewed contain indirect evidence upon this problem. A few of the more direct treatments will be brought together at this point. The issue involved is one which reaches far beyond itself and implies major systematic points of view. Insight, as Wheeler (**181**) uses it, is a purely descriptive term for organized response at the level of conscious behavior. Association does not explain it, but is a result of it. Insight depends upon the learner's level of maturation and is induced by the stimulus-pattern. Maier (**107**) asserts that in the solution of a relatively complicated problem trial-and-error may be present, but it cannot explain the sudden appearance of the correct solution when the solution requires productive thinking. Mere conscious presence of the necessary data is insufficient. A factor of "direction" must be present to organize the data. Brainard (**47**) has put a young child in problem situations analogous to those used by Köhler with apes. He notes that a pause, suggestive of insight, usually occurred just before the correct solution. Such a pause, however, may sometimes appear before an incorrect solution.

Dunkelberger and Rumberger (**38**) gave individually to the members of two groups of subjects the problem of building a set of blocks upon a base to reach two horizontal bars. Only one arrangement would solve the problem. The members of the insight group were permitted to handle the blocks as much as they pleased but were not

Trial Error

allowed to build with them until they had consummated a plan. They were then given one trial at building. The subjects in the trial-and-error group were allowed to build overtly until the problem was solved or until their inability to solve it was demonstrated. There was a much greater percentage of solutions on the part of the trial-and-error groups. The "insight" groups showed relatively little evidence of insight under the conditions employed. One wonders if continued trials on the part of this group might not have revealed changing results. In Freeman's (47) experiment, insight, in the sense of a particular attitude or way of taking the chords to be associated with reaction keys, exerted a determining influence over the items to be connected. Since the organization of reactions and chords into a structure did not occur in the absence of a specific determination, it is concluded that this attitude or insight is a primary condition of the formation of the structure.

In opposition to the introduction of an insight factor Hunter (78) writes: "The theory that there is present in some learning a factor, designated as insight, which is not of the same order as the factors involved in the conditioning of reflexes or in so-called trial-and-error learning" is not adequately supported by the facts. There seems no need for the assumption that there is more than one kind of learning. Thurstone (166) suggests that in insight the errors are eliminated without being made overtly. A considerable amount of trial-and-error is present in the solution by young children of the three non-verbal tests used by Harter (62). Insight, according to Guthrie (59), is the result of accumulated habit.

Ogden (12) contends that the problem of learning is that of organization, which cannot be made understandable on an elementaristic basis. According to Gestalt theory, learning is all of a piece, though having differential aspects, and takes place as the result of a dynamic interplay of forces which disturb equilibrium. Differentiated acts are performed by the organism as a whole. The main objection, according to R. R. Scott (144), to the Thorndikian laws of learning is their mechanistic character. Learning is, on the contrary, learning to think and the purposive character of behavior redeems even fixed habits from a mere mechanical repetition. Purposive behavior is held to be explained in terms of configurations and their properties.

The Form of the Laws of Learning. It is Hughes' (70) thesis that the underlying error in the psychology of learning is the giving to our laws the form of *necessary consequence,* where only the form of

necessary antecedence is warranted. Thence flows the present "confusion." The difference between the laws of physics and those of psychology is one of kind. The latter can state antecedents, but cannot predict necessary consequents. Brown's (**14**) summary of the methods of Lewin contains a large amount of material bearing on *Gestalt* interpretations of learning. Lewin holds that psychologists have confused the phenomenal and the conditional-genetic aspects of acts. Real laws are descriptions of the latter character. It is necessary to refer back to definite sources of energy, which Lewin finds in psychic tensions. These tensions tend to approach equilibrium as a whole. This conception has led to new experiments, some of them, as those of Zeigarnik (1927), of Schwarz (1927), and of Ovsiankina (**123**), upon learning of the skill type. The work of the two first mentioned investigators has been cited in previous reviews. Ovsiankina reports that subjects show no tendency to resume an act already completed, but that when they are interrupted in the execution of simple acts they either resume them or show a tendency to do so in 83 per cent of the cases. The resumption is not a function of the subject's interest in the act, but of the structure of the act, of the phase in which interrupted, and of the "set" of the subject. It is referable to the discharge of a derived need, which is the outcome of an unresolved psychic tension.

The Conditioned Response as a Principle of Learning. This section of the review will include papers which treat the conditioned response as a general principle, or as the chief paradigm of learning. The experimental facts, which constitute a separate problem, are not reviewed. In a series of three papers Hull (**71, 72, 73**) has laid the basis for a generalized conditioned response treatment, not only of learning but of the whole of psychology. The conditioned response is a two-phase phenomenon. Its primary phase is positive or excitatory; functionally it is a tentative trial or approach to an adaptive process. The second phase is negative or inhibitory; functionally it is selective and corrective. "These two phases of the conditioned reflex, operating jointly, thus stand revealed as an automatic trial-and-error mechanism which mediates, blindly but beautifully, the adjustment of the organism to a complex environment." In the second paper he proposes twelve questions concerning the process of learning and answers them in terms of an hypothetical example. For an adequate comprehension of this the original paper must be consulted in detail. If the principles which he enumerates are sound, it should be possible, he reasons, to construct a machine which would

manifest qualities of intelligence, insight, purpose and the like. Such a "psychic machine" has been built by Baernstein and Hull (3). In the third paper of the series mentioned, Hull gives a diagrammatic formulation of the way in which the world imprints itself on the organism after the paradigm of the conditioned response. This functional copy of the world sequence is knowledge. The general plausibility of the deductions made suggests to him the importance of an experimental program to test them.

Rexroad (136) has worked out in his text a thoroughgoing application of the conditioned response principle to the problems of learning. His statement of the necessary requirement for conditioning is a marked improvement upon the usual one: "A stimulus which repeatedly precedes a reaction, whatever the cause of that reaction, becomes capable of evoking it." Guthrie (59) takes as fundamental the principle that "stimuli which accompany a response tend, on their recurrence, to evoke that response," and proposes to reduce the other phenomena of learning to this law. The Gestaltists, he thinks, have not examined sufficiently the possibilities of proprioceptive stimuli as determiners of behavior, and have tended wrongly to place the burden of explanation on speculative properties of the cortex. According to Humphrey (77) the conditioned response is a maze experiment cut down to zero; or the maze is a conditioned response with a highly expanded secondary stimulus. Filter and Held (44) employ the conditioned response as the main principle of learning; and Watson (178) reiterates that each unit of a complex habit is a conditioned response. Hunter (78) is of the opinion that all learning is fundamentally of the conditioned response type. The conditioned response is the mechanism underlying the part-whole substitution involved in learning, according to Warren and Carmichael (176). It is one of the major points of Holt's (68) book that the conditioned response, which is only a special case of the law of neurobiotaxis, is the true psychological law of learning, "that it is, indeed, the conditioned reflex which brings *mind* into being."

Stagner (155) has written a vigorous defense of the conditioned response theory. From a psychological point of view five criticisms have been offered against it: its variability and impermanence and the number of repetitions required to establish it; the oversimplification of the concept; the dying out after some time without reinforcement; the readiness of experimental extinction; and the rapid onset of abnormal conditions. He proposes to show that these factors are also characteristic of animal and human learning, at a stage before com-

plication becomes so great as to make analysis impossible. In early learning, as in the case of a child learning to read, dropping out of reactions is very frequent. Oversimplification is true only in certain theoretical formulations. Conditioned responses may be reinstated by the establishment of new ones and by extraneous stimuli, conditions constantly present in animal and human learning. Experimental extinction appears on plateaus, in the elimination of blind alleys, in Dunlap's method of breaking habits. Many abnormal phenomena in human subjects resemble those demonstrated by Pavlov. The neural basis of the conditioned response is also discussed. Stagner does not hold that all learning is simply conditioning in the sense that it is passive and externally determined; motives and internal conditions in general are effective.

Opposed to these relatively enthusiastic proponents of the generality of the conditioned response are several writers who are at least skeptical. Thorndike (**165**) interprets the conditioned response to be a special case rather than the basic principle of learning. On their face, but not when more deeply considered, the facts of conditioning seem to contradict his findings upon the efficacy of frequency. Conditioned responses are, *e.g.,* established with no belonging and with no demonstrable satisfyingness, and after a very few trials. There are, however, many features of the conditioned response such as recovery through disuse and experimental extinction, for example, which have no counterpart in ordinary learning and which seem to give it the status of a special case. He ventures the suspicion that the conditioned response will teach us more about excitability than about learning. Lashley (**98**) writes: "There seems to be as much evidence for Poppelreuter's conclusion that all learning is of the type of logical memory, as for the opposed view that all learning is the formation of few or more conditioned reflexes."

Williams (**184**) believes that the conditioned response as an explanation of learning is unacceptable. It must rest its case on its adequacy as a descriptive concept, and the case is incomplete. It takes account only of substitution of stimuli and cannot cover acquisition of new responses; nor can it include the establishment of preparatory responses upon subsequent consummatory ones. The essential feature of learning is "the contingency of the learned response upon a subsequent response. This relation of preparatory stimulus to consummatory stimulus can be identified and described as the 'sign' function, and its recognition will further a scheme of real explanation of the learning process." In the paper mentioned,

Stagner has attempted to show that the data used by Williams can be brought within a conditioned response formulation. The usual criterion of conditioning is (Stephens, **159**) that the response which follows both conditioned and unconditioned stimulus is the same. If the response is not the same, we have trial-and-error. It is his contention that this criterion is arbitrary, will not hold in its own field, and that the identity of response in conditioning is a logical result of trial-and-error. His positive theory has already been stated in reviewing the work on the law of effect. While he considers it premature to speculate whether the conditioned response can explain skill, Pear (**126**) notes that the former is built up best in a controlled environment, the latter "typically shows itself in the rapid adjustment to a changing environment and to unforeseen conditions."

It is to be expected that subscribers to configurational principles should find the conditioned response an inadequate principle. Ogden (**121**) styles it "naïvely incomplete." The problem of learning is that of organization, and even were the juxtaposition of two items invariably to result in a connection, which is not the case, we should still not comprehend the connection. Wheeler (**181**), likewise, rejects it, because "the acquisition of new responses involves the perceiving of objects in their relationships, or in other words the construction of new perceptual configurations!" Statement in conditioned response terms tells nothing about this organization.

The major theoretical differences in the publications of the period are of the order of those just reviewed. The chief question at issue is whether learning is to be treated as a connecting of elements to give an integration, or whether organization is to be considered something more than a sum of elements and used as a principle of explanation in its own right. The discussions of the problem fail, at least to the reviewer, to indicate a hopeless confusion of the kind in which some of the recent commentators upon the status of psychology seem to think that we wander. Solidly, beneath the arguments, fact accumulates. Upon interpretation of the facts men differ. But the differences inspire fresh experimental work which, in turn, modifies the differences. So long as we all agree to start from the laboratory, varied theoretical destinations do not spell hopelessness.

The Nervous System and Learning. Lashley's (**98**) chapter in the *Foundations of Experimental Psychology* is an excellent summary of work on nervous mechanisms in learning. His presidential address (**99**) to the American Psychological Association is, likewise, an able discussion of basic neural mechanisms in behavior. He con-

cludes that a point has been reached "where the reflex theory is no longer profitable either for the formulation of problems or for an understanding of the phenomena of integration." Lashley's interpretation of his own research upon cerebral function is too well known and too important to merit brief summary. He thinks at present, however, that it is more important not to be handicapped by a false theory than to have a correct one. In point of actual knowledge about the mechanism of learning it is doubtful if we know more than Descartes did. This does not mean that the research since 1885 has been futile. "Psychology is today a more fundamental science than neurophysiology." This is specific recognition of what seems to the reviewer a neglected but evident fact.

Hunter (**79**) does not believe that Lashley's theory of the equipotentiality of the cerebral cortex is justified by the facts concerning the sensory control of the maze habit and by the data on other habits. Lashley (**100**) has recently published a reply to Hunter in which he defends his own interpretations.

Holt's (**68**) book is essentially a neurophysiological theory of mental organization. The basic principles are Bok's law of stimulogeneous fibrillation and Kappers' law of neurobiotaxis. These, supported by the reflex-circle and related concepts, are alleged to account for the whole of learning.

III. Retention

The two preceding reviews of the acquisition of skill have remarked upon the very small amount of research which is being done upon the retention of skill. This general neglect of the problem continues. Not only is little experimental research done upon it, but theorists almost unanimously fail to consider it. And yet it is at least a worthwhile hypothesis that a more adequate knowledge of the fate of acquisitions in time and the conditions which affect them would be of great importance to theories of learning, as well as for its own sake.

The Curve of Retention. Chappell (**21**) gives an equation for the curve of forgetting which dispenses with chance and rests on the laws governing inorganic events. Large numbers of physical phenomena, such as the loss of charge from a condenser and loss of head from a water tank, follow a logarithmic function. Twenty-four children who had learned the maze employed by McGinnis (**111**) relearned after intervals ranging from 22 to 195 days. They fall into groups, one averaging 42 and the other 159 days. The time and error

saving scores for the first group are 63.4 and 77.3 per cent, those for the second group are 32.3 and 31.5 per cent, respectively. Fernberger (43) reports an interesting case of long-time retention. A subject who, five years before, had served in a lifted-weight experiment served again in a similar one. The precision of judgment had not changed to any great degree, and the average for the interval of uncertainty was similar to the values after the former progressive practice. In Ewert's (42) work upon the acquisition of a new spatial coördination, much of the newly acquired behavior did not persist in measurable form after removal of the lenses. That which was retained is ascribed to a persistence of the eye-body movements necessitated by inverted vision.

The Influence of Overlearning. Krueger (93) has studied the retention values of 50 and 100 per cent overlearning of a finger maze, when one errorless trial is used as the criterion of 100 per cent learning and when per cent of overlearning is computed in terms of the trials required to reach the criterion of learning. Intervals of 1, 2, 3, 4, 7 and 14 days were used. As degree of learning increased from 100 to 150 per cent, there was an increase in all retention scores after all intervals, and this increase was always less than 50 per cent. As learning increased from 150 to 200 per cent, the corresponding increase in retention scores was usually more than a third, particularly for the longer intervals. Increase from 150 to 200 per cent is proportionally more economical than increase from 100 to 150 per cent. A second experiment showed that the increase from 100 to 200 per cent overlearning is not as economical as that from 50 to 100 per cent. No consistent variation in retention with length of time interval was found. The data indicate that the mazes used were easy to master. It would be useful to know similar facts for hard mazes.

The Influence of the Nature of the Material. It is a well known generalization that skilled acts are better retained than are materials classified as of the memorial order. McGeoch and Melton (109) conclude, after an examination of the literature on the problem, that acts of skill, while showing a greater loss with time than is usually supposed to occur, are retained much better than are nonsense syllables. More meaningful materials are retained nearly, if not quite, as well as are skills. They have made an experimental comparison of the retention, after one week, of stylus mazes of three degrees of difficulty with nonsense syllable lists of 8, 12 and 16 items. Each was learned to a criterion of one perfect trial and relearned to one of three perfect trials in succession. The relative retention values of

the two materials are functions of the maze and list compared, of the method of measuring retention, and of the relearning criterion employed. Neither material shows any consistent superiority and thus no evidence appears in support of the generalization that skilled acts are the better retained, when skill and memory materials are learned to a uniform criterion.

In the McGeoch and Melton experiment, the materials used differed radically in structure, as Freeman and Abernethy (46) have noted, and no measurement of retention was made beyond a one-week interval. Freeman and Abernethy have performed an experiment which takes account of both of these factors. One group of their subjects learned to type a short paragraph which contained every letter of the alphabet. The keys were covered with blank caps and a diagram of the keyboard was before the learner. Another group translated the same paragraph into digits following the same key as that used by the group which practiced typing, with letters numbered according to position on the keyboard. In both types of learning the movements required are the same, but the typing involves a greater amount of overt movement. Both groups relearned two weeks after the original learning and again after a total of ten weeks. In percentage of saving after two weeks the two groups are virtually equal, thus corroborating the McGeoch and Melton results. After ten weeks, however, the typewriting is retained considerably better than is the substitution. The authors conclude that the presence of overt movements facilitates retention over a long period, although it does not measurably do so over a shorter one. This experiment is a highly important attack upon the problem, although unfortunately the experimental conditions render uncertain an interpretation of the results after the ten weeks period. The groups which relearned after ten weeks had also relearned after two weeks. The fact of the difference in saving after ten weeks may be, as Freeman and Abernethy think, a function of the greater time interval, or it may be that a relearning, which constitutes distributed practice, after two weeks is more efficacious for fixating typewriting for later retention than it is in fixating substitution. Either conclusion would be of the first importance, but it is impossible to decide, on account of the conditions, which one to accept.

A conditioned galvanic reflex, established in an infant who was studied from the seventh to the ninth month, was shown by Jones (87) to persist, in spite of frequent extinction of the response, for at least seven weeks without reinforcement.

The Retention and Recognition of Maze Patterns. T. C. Scott (146) finds that a maze pattern is retained, and functions in relearning, particularly when a second maze has not been interpolated. Frequently the pattern functions unconsciously and the subject fails to recognize that he is relearning the same or a similar maze. Recognition of pattern is largely a matter of mental set. The verbal method of learning is found to be the best, the motor method to be poorest.

The Influence of Guidance. Waters'(177) study of the influence of large amounts of manual guidance upon stylus maze learning has already been referred to. The influence upon retention was also studied. In terms of saving, with only the learning records subsequent to guidance included in the denominator, the control group retains the habit better than do the guided groups. When the guided trials are included as a part of learning, saving in trials and time increases slightly with the amount of the guidance, and in terms of trials the subjects with the larger amounts of guidance retain the habit better than do the controls. When absolute recall, the first relearning trial, is considered, the guided groups are superior in terms of errors, while in terms of time the groups given 20 and 40 guided trials are superior to the controls and the group given 80 guided trials is inferior. Final speed is greater for the controls.

The Influence of Specific Training. Morgan (115) inquires whether special training on the basis of diagnostic test results affects retention of arithmetic, silent reading, and problem solving. It is concluded that the special training acts as a buffer against forgetting.

Length of Problem and Retention of Rational Learning. McGeoch and Oberschelp (110) have studied the relative retention values, after 7 days, of Paterson Rational Learning problems of 6, 12 and 18 letters. All measures of retention support the conclusion that increasing length is accompanied by increasing retention which is not, however, in proportion to the lengths of the problems. A comparison of the saving scores with those obtained in the McGeoch and Melton experiment shows that the rational learning solutions are retained considerably better than are either mazes or nonsense syllables.

Conditioning as an Explanation of Forgetting. In opposition to the usual textbook statements that learning is dissipated by some physiological change at the synapse, Guthrie (59) suggests that forgetting is better explained by the theory that stimuli which were conditioners of the activity in question become in the meantime components of new situations. The form of the curve of forgetting may

represent a piling up of the alienation of conditioners from their responses. Retroactive inhibition is explicable in similar terms.

BIBLIOGRAPHY

1. Anderson, L. D., A Preliminary Study of the Effect of Training in Junior High School Shop Courses. *Arch. of Psychol.*, 1929, No. 109, pp. 39.
2. Archer, C. P., Transfer of Training in Spelling. *Univ. Iowa Stud., Stud. in Educ.*, 1930, **5**, No. 5, pp. 63.
3. Baernstein, H. D., and Hull, C. L., A Mechanical Model of the Conditioned Reflex. *J. Gener. Psychol.*, 1931, **5**, 99–106.
4. Baldwin, M., and Ewert, P. H., A Study of Motor Activity in Three Pairs of Identical Twins. *J. Genet. Psychol.*, 1930, **38**, 458–466.
5. Beeby, C. E., An Experimental Investigation into the Simultaneous Constituents in an Act of Skill. *Brit. J. Psychol.*, 1930, **20**, 336–353.
6. Bloor, C., *The Process of Learning: Some Psychological Aspects of Learning and Discipline in School.* London: Kegan Paul, 1930.
7. Boder, D. P., A Tridimensional Maze. *Amer. J. Psychol.*, 1930, **42**, 107–108.
8. Book, W. F., How a Special Disability in Spelling was Diagnosed and Corrected. *J. Appl. Psychol.*, 1929, **13**, 378–393.
9. Boring, E. G., *A History of Experimental Psychology.* New York: Century Co., 1929.
10. Bowman, H. L., The Relation of Reported Preference to Performance in Problem Solving. *Univ. Mo. Bull.*, 1929, **30**, pp. 52.
11. Brainard, P. P., The Mentality of a Child Compared with that of Apes. *J. Genet. Psychol.*, 1930, **37**, 268–292.
12. Brian, C. R., and Goodenough, F. L., The Relative Potency of Color and Form Perception at Various Ages. *J. Exper. Psychol.*, 1929, **12**, 197–213.
13. Brown, G. G., Perception of Depth with Disoriented Vision. *Brit. J. Psychol.*, 1929, **19**, 117–146.
14. Brown, J. F., The Methods of Kurt Lewin in the Psychology of Action and Affection. *Psychol. Rev.*, 1929, **36**, 200–221.
15. Bruce, W. F., Shall "Habits" be Discarded in Educational Psychology? *J. Educ. Psychol.*, 1930, **21**, 479–488.
16. Bruce, W. F., Habits, Intelligence and Analysis. *J. Educ. Psychol.*, 1930, **21**, 574–585.
17. Brümmer, F., A Mechanical Ingenuity Test. *J. Gener. Psychol.*, 1929, **2**, 522–526.
18. Carmichael, L., A Relationship Between the Psychology of Learning and the Psychology of Testing. *Sch. & Soc.*, 1930, **31**, 687–693.
19. Carr, H., Teaching and Learning. *J. Genet. Psychol.*, 1930, **37**, 189–218.
20. Chaisson, A. F., An Alternative Approach to the Mathematical Study of Learning Curves. *J. Gener. Psychol.*, 1930, **4**, 352–359.
21. Chappell, M. N., Chance and the Curve of Forgetting. *Psychol. Rev.*, 1931, **38**, 60–64.
22. Chevalier, J., *L'Habitude.* Paris: Boivin, 1929.

23. Chou, S. K., Reading and Legibility of Chinese Characters. *J. Exper. Psychol.*, 1929, **12**, 156–177.

24. Chou, S. K., A Multiple Groove Board for Testing Motor Skill. *J. Exper. Psychol.*, 1929, **12**, 249–253.

25. Chou, S. K., A Universal Finger Maze: the Pattern Box. *J. Gener. Psychol.*, 1929, **2**, 527–532.

26. Corey, S. M., Certain Ambiguous Terms in Educational Psychology. *J. Educ. Psychol.*, 1931, **22**, 131–138.

27. Crafts, L. W., Whole and Part Methods with Non-serial Reactions. *Amer. J. Psychol.*, 1929, **41**, 543–563.

28. Crafts, L. W., Whole and Part Methods with Unrelated Reactions. *Amer. J. Psychol.*, 1930, **42**, 591–601.

29. Crews, H. R., An Experiment with Finger Gymnastics in Teaching Typewriting. Research Studies in Commercial Education, III. Collected by E. G. Blackstone. *Univ. Iowa Monog. Educ.*, 1928, First Series, No. 9, 126–144.

30. Crosland, H. R., Taylor, H. R., and Newsom, S. J., Practice and Improvability in the Müller-Lyer Illusion in Relation to Intelligence. *J. Gener. Psychol.*, 1929, **2**, 290–304.

31. Cureton, E. E., A Modification of Thomson's Formula for the Correlation between Initial Status and Gain, and its Standard Error. *J. Exper. Psychol.*, 1930, **13**, 358–364.

32. Dallenbach, K. M., A Reply to Dr. Hollingworth. *Amer. J. Psychol.*, 1930, **42**, 458–459.

33. Dashiell, J. F., Some Simple Apparatus for Serial Reactions. *J. Exper. Psychol.*, 1930, **13**, 352–357.

34. Dehove, H., Le problème de l'habitude et les facultés de l'âme. *Rev. de Phil.*, 1929, **27**, 188–203.

35. Deputy, E. C., Knowledge of Success as a Motivating Influence in College Work. *J. Educ. Res.*, 1929, **20**, 327–334.

36. Dorsey, M. F., and Hopkins, L. T., The Influence of Attitude upon Transfer. *J. Educ. Psychol.*, 1930, **21**, 410–417.

37. Dunford, R. E., The Genetic Development of Cutaneous Localization. *J. Genet. Psychol.*, 1930, **37**, 499–512.

38. Dunkelberger, G. F., and Rumberger, E. K., An Experimental Study in Perceptual Insight. *J. Gener. Psychol.*, 1930, **4**, 385–389.

39. Dunlap, K., Repetition in the Breaking of Habits. *Scient. Mo.*, 1930, **30**, 66–70.

40. Efimoff, W., Sarch, M., and Krasnikowa, J., Übung und Ermüdung der Arbeiter bei Fliessarbeit. *Zsch. f. angew. Psychol.*, 1931, **38**, 120–135.

41. Entwisle, B. S., An Experiment with Rhythm in Teaching Typewriting. Research Studies in Commercial Education, II. Collected by E. G. Blackstone. *Univ. Iowa Monog. Educ.*, 1928, First Series, No. 8, 75–83.

42. Ewert, P. H., A Study of the Effect of Inverted Retinal Stimulation upon Spatially Coördinated Behavior. *Genet. Psychol. Monog.*, 1930, **7**, 177–361.

43. Fernberger, S. W., Constancy of Attitude Maintained over a Long Period of Time. *Amer. J. Psychol.*, 1930, **42**, 317–318.

44. FILTER, R. O., and HELD, O. C., *The Growth of Ability.* Baltimore: Warwick and York, 1930.

45. FRANZ, S. I., and WATSON, T. A., A Recording Pencil Maze. *J. Gener. Psychol.*, 1929, **2**, 515–517.

46. FREEMAN, F. N., and ABERNETHY, E. M., Comparative Retention of Typewriting and of Substitution with Analogous Material. *J. Educ. Psychol.*, 1930, **21**, 639–647.

47. FREEMAN, G. L., The Influence of Attitude on Learning. *J. Gener. Psychol.*, 1930, **3**, 98–111.

48. FREIBERG, A. B., DALLENBACH, K. M., and THORNDIKE, E. L., The Influence of Repetition of a Series upon the Omission of its Intermediate Terms. *Amer. J. Psychol.*, 1929, **41**, 637–639.

49. GARIAYEVA, R., Sur la loi de l'éducabilité. *Arch. de Psychol.*, 1930, **22**, 144–152.

50. GARRETT, H. E., *Great Experiments in Psychology.* New York: Century Co., 1930.

51. GARRISON, K. C., An Investigation of Some Simple Speed Activities. *J. Appl. Psychol.*, 1929, **13**, 167–172.

52. GEMELLI, A., Richerche sperimental sulla natura e sulla diagnosi dell'abilita manuale. Milan: Societa editrice "Vita e pensiero," 1929, pp. 54.

53. GEMELLI, A., Recherches sur la nature l'habilité manuelle. *J. de Psychol.*, 1929, **25**, 163–200.

54. GESELL, A., Maturation and Infant Behavior Pattern. *Psychol. Rev.*, 1929, **36**, 307–319.

55. GESELL, A., and THOMPSON, H., Learning and Growth in Identical Infant Twins. *Genet. Psychol. Monog.*, 1929, **6**, 1–120.

56. GLAZE, J. A., The Effects of Practice on Fatigue. *Amer. J. Psychol.*, 1930, **42**, 628–630.

57. GRAY, J. S., A Behavioristic Interpretation of Concept Formation. *Psychol. Rev.*, 1931, **38**, 65–72.

58. GUILFORD, J. P., An Experiment in Learning to Read Facial Expression. *J. Abn. & Soc. Psychol.*, 1929, **24**, 191–202.

59. GUTHRIE, E. R., Conditioning as a Principle of Learning. *Psychol. Rev.*, 1930, **37**, 412–428.

60. HAMILTON, H. C., The Effect of Incentives on Accuracy of Discrimination Measured on the Galton Bar. *Arch. of Psychol.*, 1929, **16**, No. 103, pp. 73.

√ 61. HARDY, M. C., The Effect of Distribution of Practice in Learning a Stylus Maze. *J. Comp. Psychol.*, 1930, **10**, 85–96.

62. HARTER, G. L., Overt Trial and Error in the Problem Solving of Preschool Children. *J. Genet. Psychol.*, 1930, **38**, 361–371.

√ 63. HENSHAW, E. M., and HOLMAN, P. G., A Note on Over-training. *Brit. J. Psychol.*, 1930, **20**, 333–335.

64. HICKS, J. A., The Acquisition of Motor Skill in Young Children. *Child Development*, 1930, **1**, 90–105.

65. HOLLINGWORTH, H. L., On the Omission of Intermediate Acts. *Amer. J. Psychol.*, 1930, **42**, 457–458.

66. HOLLINGWORTH, H. L., Effect and Affect in Learning. *Psychol. Rev.*, 1931, **38**, 153–159.

67. Holsopple, J. Q., and Feldstein, M. J., A Simple Statistical Method for Psychology. *J. Gener. Psychol.*, 1930, **3**, 157–166.

68. Holt, E. B., *Animal Drive and the Learning Process*, Vol. I. New York: Holt & Co., 1931.

69. Holzinger, K. J., The Probable Error of a Difference Formula. *J. Educ. Psychol.*, 1930, **21**, 63–64.

70. Hughes, P., Coöperation or Conflict in the Study of Learning. *Psychol. Rev.*, 1930, **37**, 350–360.

71. Hull, C. L., A Functional Interpretation of the Conditioned Reflex. *Psychol. Rev.*, 1929, **36**, 498–511.

72. Hull, C. L., Simple Trial-and-Error Learning: a Study in Psychological Theory. *Psychol. Rev.*, 1930, **37**, 241–256.

73. Hull, C. L., Knowledge and Purpose as Habit Mechanisms. *Psychol. Rev.*, 1930, **37**, 511–525.

74. Hull, C. L., Quantitative Methods of Investigating Hypnotic Suggestion, Part I. *J. Abn. & Soc. Psychol.*, 1930, **25**, 200–223.

75. Hull, C. L., Quantitative Methods of Investigating Hypnotic Suggestion, Part II. *J. Abn. & Soc. Psychol.*, 1931, **25**, 390–417.

76. Humphrey, G., Extinction and Negative Adaptation. *Psychol. Rev.*, 1930, **37**, 361–363.

77. Humphrey, G., Learning and the Living System. *Psychol. Rev.*, 1930, **37**, 497–510.

78. Hunter, W. S., Learning: II. Experimental Studies of Learning. Chap. 15 in *The Foundations of Experimental Psychology*, Ed. by C. Murchison. Worcester: Clark Univ. Press, 1929, pp. 564–627.

79. Hunter, W. S., A Consideration of Lashley's Theory of the Equipotentiality of Cerebral Action. *J. Gener. Psychol.*, 1930, **3**, 455–468.

80. Husband, R. W., A Comparison of Human Adults and White Rats in Maze Learning. *J. Comp. Psychol.*, 1929, **9**, 361–377.

81. Husband, R. W., A Note on Maze Learning with the Time Factor Constant. *J. Gener. Psychol.*, 1929, **2**, 366–369.

82. Husband, R. W., Certain Age Effects on Maze Performance. *J. Genet. Psychol.*, 1930, **37**, 325–328.

83. Husband, R. W., Semi-logarithmic versus Linear Plotting of Learning Curves. *J. Educ. Psychol.*, 1931, **22**, 72–75.

84. Israeli, N., Illusions in the Perception of Short Time Intervals. *Arch. of Psychol.*, 1930, No. 113, pp. 47.

85. James, H. E. O., The Transfer of Training. *Brit. J. Psychol.*, 1930, **20**, 322–332.

86. Jasper, H. H., Is Perseveration a Functional Unit Participating in All Behavior Processes? *J. Soc. Psychol.*, 1931, **2**, 28–50.

87. Jones, H. E., The Retention of Conditioned Emotional Reactions in Infancy. *J. Genet. Psychol.*, 1930, **37**, 485–497.

88. Jones, H. E., The Conditioning of Overt Emotional Responses. *J. Educ. Psychol.*, 1931, **22**, 127–130.

89. Kern, B., *Wirkungsformen der Übung: Beiträge zur Psychologie der Arbeit.* Münster: Helios-Verlag, 1930.

90. Kern, B., and Lindow, M., Die mathematische Auswertung empirisch

gefundener Kurven mit besonderer Berücksichtigung der Übungskurven. *Zsch. f. angew. Psychol.*, 1930, **35**, 497–529.

91. KLEMM, O., Zufall oder Geschicklichkeit. *Neue Psychol. Stud.*, 1929, 5, 23–64.

92. KNOTTS, J. R., and MILES, W. R., The Maze-learning Ability of Blind Compared with Sighted Children. *J. Genet. Psychol.*, 1929, **36**, 21–49.

93. KRUEGER, W. C. F., Further Studies in Overlearning. *J. Exper. Psychol.*, 1930, **13**, 152–163.

94. LAIRD, D. A., and MULLER, C. G., *Sleep.* New York: John Day Co., 1930.

95. LANDIS, C., Emotion: II. The Expressions of Emotion. Chap. 13 in *The Foundations of Experimental Psychology.* Ed. by C. Murchison. Worcester: Clark Univ. Press, 1929, pp. 488–523.

96. LANIER, L. H., An Analysis of Thinking Reactions of White and Negro Children. *J. Comp. Psychol.*, 1930, **10**, 207–220.

97. LANIER, L. H., Review of Wheeler, R. H., *The Science of Psychology. Amer. J. Psychol.*, 1931, **43**, 146–150.

98. LASHLEY, K. S., Learning: I. Nervous Mechanisms in Learning. Chap. 14 in *The Foundations of Experimental Psychology.* Ed. by C. Murchison. Worcester: Clark Univ. Press, 1929, pp. 524–563.

99. LASHLEY, K. S., Basic Neural Mechanisms in Behavior. *Psychol. Rev.*, 1930, **37**, 1–24.

100. LASHLEY, K. S., Cerebral Control versus Reflexology: a Reply to Professor Hunter. *J. Gener. Psychol.*, 1931, **5**, 3–19.

101. LEFEVRE, L. J., Habitude et habitus. *Rev. de Phil.*, 1928, **28**, 435–466.

102. LEONARD, J. P., The Use of Practice Exercises in Teaching Capitalization and Punctuation. *J. Educ. Res.*, 1930, **21**, 186–190.

103. LEUBA, C. J., A Preliminary Analysis of the Nature and Effects of Incentives. *Psychol. Rev.*, 1930, **37**, 429–440.

104. LEUBA, C. J., A Preliminary Experiment to Quantify an Incentive and its Effects. *J. Abn. & Soc. Psychol.*, 1930, **25**, 275–288.

√ 105. LORGE, I., Influence of Regularly Interpolated Time Intervals upon Subsequent Learning. *T. C. Contr. Educ.*, 1930, No. 438, pp. 57.

√ 106. LUH, C. W., and LIANG, B. T., An Experiment in Continuous Dart-throwing. *J. Genet. Psychol.*, 1930, **38**, 450–458.

107. MAIER, N. R. F., Reasoning in Humans. I. On Direction. *J. Comp. Psychol.*, 1930, **10**, 115–143.

108. MARQUIS, D. G., The Criterion of Innate Behavior. *Psychol. Rev.*, 1930, **37**, 334–349.

109. MCGEOCH, J. A., and MELTON, A. W., The Comparative Retention Values of Maze Habits and of Nonsense Syllables. *J. Exper. Psychol.*, 1929, **12**, 392–414.

110. MCGEOCH, J. A., and OBERSCHELP, V. J., The Influence of Length of Problem and of Transfer upon Rational Learning and its Retention. *J. Gener. Psychol.*, 1930, **4**, 154–168.

√ 111. MCGINNIS, E., The Acquisition and Interference of Motor Habits in Young Children. *Genet. Psychol. Monog.*, 1929, **6**, 203–311.

112. MENDENHALL, J. E., The Characteristics of Spelling Errors. *J. Educ. Psychol.*, 1930, **21**, 648–656.

113. Merry, R. V., Which Hand is the Eye of the Blind? *Teach. Forum (Blind)*, 1930, **2**, 4–6.

114. Miles, W., A One-handed Motor Task. *J. Gener. Psychol.*, 1929, **2**, 145–146.

115. Morgan, L. D., How Effective is Specific Training in Preventing Loss Due to the Summer Vacation? *J. Educ. Psychol.*, 1929, **20**, 466–471.

116. Murphy, G., *An Historical Introduction to Modern Psychology.* New York: Harcourt, Brace, 1929.

117. Muse, M. B., *An Introduction to Efficient Study Habits According to the Laws and Principles Governing Economical Learning.* Philadelphia: Saunders, 1929.

118. Nathanson, Y. S., A "Conceptual" Basis of Habit Modification. *J. Appl. Psychol.*, 1929, **13**, 469–485.

119. Nichols, M. L., The High School Student and Scientific Method. *J. Educ. Psychol.*, 1929, **20**, 196–204.

120. Nyswander, D. B., A Comparison of the High Relief Finger Maze and the Stylus Maze. *J. Gener. Psychol.*, 1929, **2**, 273–288.

121. Ogden, R. M., The Gestalt Psychology of Learning. *J. Genet. Psychol.*, 1930, **38**, 280–286.

122. Overman, J. R., An Experimental Study of the Effect of the Method of Instruction on Transfer in Arithmetic. *Elem. Sch. J.*, 1930, **31**, 183–190.

123. Ovsiankina, M., Die Wiederaufnahme unterbrochener Handlungen. VI in K. Lewin's Untersuchungen zur Handlungs- und Affectpsychologie. *Psychol. Forsch.*, 1928, **11**, 302–389.

124. Panlasigui, I., and Knight, F. B., The Effect of Awareness of Success or Failure. *29th Yrbk. Nat. Soc. Stud. Educ.*, 1930, Pt. II, 611–621.

125. Pear, T. H., The Nature of Skill. *J. Nat. Instit. Indus. Psychol.*, 1928, **4**, 193–202.

126. Pear, T. H., The Nature of Skill. *Rep. Brit. Assoc. Adv. Sci.*, 1928, 168–184.

127. Pear, T. H., The Human Factor in Industry. *J. Soc. Chem. Indus.*, 1929, **48**, 227–231.

128. Pear, T. H., Some Subtler Skills. *Brit. J. Psychol.*, 1929, **20**, 145–160.

129. Pear, T. H., A Suggested Term: Euphasia. *Brit. J. Psychol.*, 1930, **20**, 371.

130. Pear, T. H., Langdon, J. N., and Yates, E. M., "Transfer of Training" and "Transference." *New Era*, 1928, July.

131. Pearson, D., An Experiment with Automatization of the 1,000 Commonest Words in Typewriting. Research Studies in Commercial Education, II. Collected by E. G. Blackstone. *Univ. Iowa Monog. Educ.*, 1928, First Series, No. 8, 84–97.

132. Peterson, J., and Allison, L. W., Effects of Visual Exposure on the Rate and Reliability of Stylus-maze Learning. *J. Gener. Psychol.*, 1930, **4**, 36–46.

133. Powers, F. F., Psychology of Language Learning. *Psychol. Bull.*, 1929, **26**, 261–274.

134. Renshaw, S., The Errors of Cutaneous Localization and the Effect of Practice on the Localizing Movement in Children and Adults. *J. Genet. Psychol.*, 1930, **38**, 223–237.

135. Renshaw, S., Wherry, R. J., and Newlin, J. C., Cutaneous Localization in Congenitally Blind versus Seeing Children and Adults. *J. Genet. Psychol.*, 1930, **38**, 239–247.

136. Rexroad, C. N., *General Psychology*. New York: Macmillan, 1929.

137. Robert, A., Recherches sur l'entrainement et l'éducabilité au point de vue professionel. *Rev. de la Sci. du Travail*, 1929, **1**, 233–256.

138. Robinson, E. S., Review of Boring, E. G., *A History of Experimental Psychology*. *Science*, 1930, **72**, 529–532.

139. Rubinstein, P., Untersuchungen über die individuelle Übungsfähigkeit bei Funktionen verschiedener Höhe. *Zsch. f. angew. Psychol.*, 1930, **37**, 417–437.

140. Sanderson, S., Intention in Motor Learning. *J. Exper. Psychol.*, 1929, **12**, 463–489.

141. Schlosberg, H., and Carmichael, L., Three New Pieces of Apparatus from the Psychological Laboratory of Brown University: a New Stylus Maze. *Amer. J. Psychol.*, 1931, **43**, 120.

142. Schorn, M., Untersuchungen über die Handgeschicklichkeit. *Zsch. f. Psychol.*, 1929, **112**, 325–378.

143. Scott, H. D., Hypnosis and the Conditioned Reflex. *J. Gener. Psychol.*, 1930, **4**, 113–129.

144. Scott, R. R., Some Suggestions on Learning from the Point of View of Gestalt Psychology. *J. Educ. Psychol.*, 1930, **21**, 361–366.

145. Scott, T. C., An Improved Form of Stylus Maze. *J. Genet. Psychol.*, 1929, **36**, 489–490.

146. Scott, T. C., The Retention and Recognition of Patterns in Maze Learning. *J. Exper. Psychol.*, 1930, **13**, 164–207.

147. Seashore, R. H., Individual Differences in Motor Skills. *J. Gener. Psychol.*, 1930, **3**, 38–65.

148. Simpson, R. G., The Effect of Specific Training on Ability to Read Historical Materials. *J. Educ. Res.*, 1929, **20**, 343–351.

149. Smith, J. M., Which Hand is the Eye of the Blind? *Genet. Psychol. Monog.*, 1929, **5**, 209–249.

150. Smith, M. D., Periods of Arrested Progress in the Acquisition of Skill. *Brit. J. Psychol.*, 1930, **21**, 1–28.

151. Smith, S., and Powers, F. F., The Relative Value of Vocabulary and Sentence Practice for Language Learning. *J. Soc. Psychol.*, 1930, **1**, 451–461.

152. Sollier, P., and Drabs, J., La prévision de la perfectibilité des aptitudes motrices est-elle expérimentablement possible? *Rev. de la Sci. du Travail*, 1929, **1**, 26–41.

153. Sollier, P., and Drabs, J., Perfectibilité et aptitudes motrices. *Rev. de la Sci. du Travail*, 1929, **1**, 513–524.

154. Spence, K. W., and Townsend, S., A Comparative Study of Groups of High and Low Intelligence in Learning a Maze. *J. Gener. Psychol.*, 1930, **3**, 113–129.

155. Stagner, R., Conditioned Reflex Theories of Learning. *Psychol. Rev.*, 1931, **38**, 42–59.

156. Stainer, W. J., A Speed-accuracy Competition. *Brit. J. Psychol.*, 1929, **20**, 82–89.

157. STEIN, M. R., A Critical Review of an Investigation in the Psychology of Suggestion and Hypnosis. *J. Abn. & Soc. Psychol.*, 1930, **25**, 49–56.

158. STEPHENS, J. M., A Mechanical Explanation of the Law of Effect. *Amer. J. Psychol.*, 1929, **41**, 422–431.

159. STEPHENS, J. M., Some Weaknesses in the Explanation of Habit Fixation as Conditioning. *Psychol. Rev.*, 1931, **38**, 137–152.

160. STRAYER, L. C., Language and Growth: the Relative Efficacy of Early and Deferred Vocabulary Training, Studied by the Method of Co-twin Control. *Genet. Psychol. Monog.*, 1930, **8**, 209–317.

161. SWIFT, S. C. (No title). *Outlook for the Blind*, 1930, **23**, 51–55.

162. SYMONDS, P. M., Practire versus Grammar in the Learning of Correct English Usage. *J. Educ. Psychol.*, 1931, **22**, 81–95.

163. TAUSCHER, E., Über die Korrelation zwischen Handgeschicklichkeit und Intelligenz. *Neue Psychol. Stud.*, 1930, **5**, 197–228.

164. TELFORD, C. W., The Refractory Phase of Voluntary and Associative Responses. *J. Exper. Psychol.*, 1931, **14**, 1–36.

165. THORNDIKE, E. L., *Human Learning*. New York: Century Co., 1931.

166. THURSTONE, L. L., The Learning Function. *J. Gener. Psychol.*, 1930, **3**, 469–491.

167. TUTTLE, H. S., Habit and Attitude. *J. Educ. Psychol.*, 1930, **21**, 418–428.

168. VALENTINE, W. L., A Graphical Method for Fitting Curves to Learning Data. *J. Gener. Psychol.*, 1930, **3**, 337–340.

169. VALENTINE, W. L., A Study of Learning Curves: II. The Relationship between the Hyperbola and the Arc Cotangent Function. *J. Gener. Psychol.*, 1930, **4**, 359–362.

170. VAUGHN, J., and DISERENS, C. M., The Relative Effects of Various Intensities of Punishment on Learning and Efficiency. *J. Comp. Psychol.*, 1930, **10**, 55–66.

171. VICKERS, W., and HOSKINS, V. H., Results from Some New Tests of Practical Abilities. *Brit. J. Psychol.*, 1929, **20**, 66–81.

172. WAKEHAM, G., A Quantitative Experiment on Dr. K. Dunlap's "Revision of the Fundamental Law of Habit Formation." *J. Comp. Psychol.*, 1930, **10**, 235–236.

173. WALLON, H., L'habilité manuelle. *Rev. de la Sci. du Travail*, 1929, **1**, 217–232.

174. WALTON, A., Demonstrational and Experimental Devices: II. Conditioning Illustrated by an Automatic Mechanical Device. *Amer. J. Psychol.*, 1930, **42**, 110–111.

175. WARDEN, C. J., A Symmetrical Linear Maze for Use in the Analysis of Animal Serial Learning. *J. Genet. Psychol.*, 1929, **36**, 177–178.

176. WARREN, H. C., and CARMICHAEL, L., *Elements of Human Psychology*. New York: Houghton Mifflin, 1930.

177. WATERS, R. H., The Influence of Large Amounts of Manual Guidance upon Human Maze Learning. *J. Gener. Psychol.*, 1930, **4**, 213–227.

178. WATSON, J. B., *Behaviorism* (Rev. Ed.). New York: Norton, 1930.

179. WEISKOTTEN, T. F., and FERGUSON, J. E., A Further Study of the Effects of Loss of Sleep. *J. Exper. Psychol.*, 1930, 13, 247–266.

180. WEISS, A. P., Review of *The Foundations of Experimental Psychology* (Ed. by C. Murchison). *J. Appl. Psychol.*, 1930, **14**, 102–103.

181. WHEELER, R. H., *The Science of Psychology.* New York: Crowell, 1929.
182. WHEELER, R. H., *Readings in Psychology.* New York: Crowell, 1930.
183. WHITE, H. D. J., Verbal Skill or "Euphasia"? *Brit. J. Psychol.,* 1930, **21**, 203.
184. WILLIAMS, K. A., The Conditioned Reflex and the Sign Function in Learning. *Psychol. Rev.,* 1929, **36**, 481–497.
185. WILSON, F. T., A Comparison of Difficulty and Accuracy in the Learning of Bright and Dull Children in a Motor-Memory Task. *J. Educ. Psychol.,* 1930, **21**, 507–511.
186. WILSON, F. T., Errors, Difficulty, Resourcefulness, and Speed in the Learning of Bright and Dull Children. *J. Educ. Psychol.,* 1931, **22**, 229–240.
187. WINSOR, A. L., Inhibition and Learning. *Psychol. Rev.,* 1929, **36**, 389–401.
188. WINSOR, A. L., Experimental Extinction and Negative Adaptation. *Psychol. Rev.,* 1930, **37**, 174–178.
189. WINSOR, A. L., Observations on the Nature and Mechanism of Secretory Inhibition. *Psychol. Rev.,* 1930, **37**, 399–411.
190. WOLFLE, H. M., Time Factors in Conditioning Finger-Withdrawal. *J. Gener. Psychol.,* 1930, **4**, 372–378.
191. YUM, K. S., An Experimental Test of the Law of Assimilation. *J. Exper. Psychol.,* 1931, **14**, 68–82.
192. ZENER, K. E., and WEVER, E. G., A Multiple-Choice Apparatus. *Amer. J. Psychol.,* 1929, **41**, 647–648.
193. ZIPF, G. K., Relative Frequency as a Determinant of Phonetic Change. *Harvard Stud. Class. Philol.,* 1929, **40**, 1–95.

SPECIAL REVIEWS

THORNDIKE, EDWARD L. *Human Learning.* New York: The Century Co., 1931. Pp. 200.

This volume comprises the fifth series of the Messenger Lectures, delivered during 1928–1929 at Cornell University. Its topic, as the lecturer remarks, "is closely and emphatically relevant to the problem of the Evolution of Civilization, specified in the donor's gift." "Man's power to change himself, that is, to learn, is perhaps the most impressive thing about him. Modern theories explaining it will help acquaint us with certain important theories of the mind as a whole" (p. 3). The first six lectures are abbreviated reports of experimental work, a full technical account of which is promised for publication elsewhere. The remaining six lectures consist of critical discussions of related work in learning and of the general implications of Professor Thorndike's own work. In some of these chapters his previous publications are drawn upon for the major part of the material, but the old is effectively placed in the setting of the new, and its restatement serves both to round out the argument and to show how the results of the present experiments may be interpreted to fit the quantity theory of mind.

With characteristic directness Professor Thorndike proceeds at once to the concepts which he proposes to employ. These concepts are fundamental to the whole book and must be understood at the outset. "Human learning," the problem of the lectures, "consists of changes in the nature and behavior of human beings. Changes in nature are known to us only by changes in behavior. The word *behavior* as used here and later means anything which the human animal does. It includes thoughts and feelings as truly as movements, and makes no assumptions concerning the deeper nature of any of these. It takes them as they are found" (p. 4). These changes in behavior take place in terms of situations and connections. A man's life may conveniently be expressed in terms of the situations he encounters, the responses which they evoke, and the connections whereby the situations lead to the responses. The implications of the term "behavior" or "response" have been stated already; "situation" is left undefined, and "connection" is used to refer to the "tendencies for a given situation to evoke certain responses rather

than others" (p. 4). (The reader will find seven other meanings listed on pp. 62–63.) The strength of a connection means the degree of probability that the situation, S_1, will be followed by the response R_{27}, for example, and learning is a matter of changing the strengths of connections. When the probability of a connection is increased from zero or an infinitesimal, it is usually said that a new connection has been formed, but since this formation of so-called new ones and the strengthening of old ones reduce to a matter of probability, both are cases of strengthening as defined. It is a real case of learning even if the probability of a connection, originally less than one in 10,000, is increased to one. No assumptions are made concerning the physiological substrate of connections. They are only expressions of probability and the words bond, link, relation, or tendency might be used as well.

This account of the nature of learning is "intentionally naïve and superficial," and some of the objections to it are listed. They are dismissed, however, as unimportant on the ground that the "facts will be true and valuable regardless of the vocabulary in which they are presented" (p. 8). A more rigorous definition of concepts is thus postponed until the facts require it. The greater generality of a bond described as a given probability than one in terms of intimacy of synaptic junction is apparent, and in this respect the present treatment improves upon the past, although interpretation eventually returns to the synapse. Whether one can be content with the ready dismissal of the difficulties which the undefined concept "situation" must meet is, perhaps, a matter of one's theoretical bias. From a purely experimental approach it has many advantages, although, systematically, the ghost of vagueness always stalks it and at many later points in the book the treatment would be strengthened by greater exactness of definition. On the response side, likewise, generality runs the risk of covering inability to specify. In the experiments on frequency and effect these difficulties are avoided rather well. In the later chapters on the more complex functions one feels more keenly the need of specification.

The question whether the mere repetition of a situation, in and of itself, produces learning is the first to be raised. Sample situations applied with relatively enormous frequencies are: (1) the instruction, "draw a four-inch line with one quick movement," was presented 3,000 times, and (2) to a long list of the beginnings of words, as *ab, ac, bo,* and the like, some of which occurred 28 times in the course of the experiment, subjects were required to add one or more letters

to make a word, performing 240 completions daily for 14 days. In the first case, the lines drawn at the last sitting were not demonstrably different from those drawn at the first. In the second, changes occurred but not in the direction of an increase in the initially frequent at the expense of the initially rare. Other experiments, not reported, yield similar results. The net conclusion is that mere frequency of situation "may change a man as little as the repetition of a message over a wire changes the wire" (p. 14). Theories of inhibition by drainage and educational doctrines which attach value to activity as such are held to be rendered very dubious by these results.

A coördinate inquiry is what changes are effected by the repeated occurrence of a connection. Long series of pairs, from 500 to 4,000, were presented with certain ones recurring often. The subject was to listen with no effort to remember, or he was asked to write the pairs under the impression that the experiment was one on fatigue or on speed and accuracy; or sentences were read to him and he was later asked to give the sequence from the end of one sentence to the beginning of the next, or of words within the sentence. The result of these and of similar experiments is that mere repetition, temporal contiguity, has very little positive effect, perhaps none, upon learning. When the response called for has been recognized by the subject as "belonging" to the stimulus and accepted by him as a proper sequence, strengthening of the connection is much greater. This principle of belonging, with acceptance of the sequence as proper, is of the first importance and has usually been neglected by students of learning. But even when belongingness and acceptability are present, mere repetition is weak. Additional factors are needed to account for learning. The one possible exception to the preceding conclusions is the conditioned response, which is treated later in the lectures.

The importance of the principle of belonging merits for it a more extended characterization. No effort is made, for example, to specify the nature of belonging. It is, apparently, as are situation and response, taken as found. Many questions, however, remain unanswered. How does belonging act upon a connection? To whom or for what does a given sequence belong? How is it identified? Obviously, if identification is in terms of better learning, the reasoning is circular. Is it the same as meaning? If so, how does meaning act? Is it to be considered as a principle of integration which includes terms and connections in a single field, or as an added element which is necessary to complete integration? In the absence of answers to

such questions, belonging becomes, not less important, but more puzzling.

The experiments upon frequency have made it apparent that learning involves more than the repetition of relevant sequences. Particularly is this clear in cases where a response frequently made in the early stages of learning is finally displaced by an initially infrequent one. Some other principle than frequency with belonging is needed, and the after-effects of a connection are invoked. In a series of experiments, situations offering the possibilities of multiple response were presented, but the consequences of the responses were varied. When, for example, a subject gave one response, he heard the word "Right," when another, he heard "Wrong." This procedure was applied to the completions of words, to vocabulary learning, and to other types of connections. The results clearly indicated great strengthening of the rewarded connection compared with others of nearly equal initial strength. In an experiment upon the estimation of areas, some one wrong response even occurred much oftener than the right one, but was eventually displaced by the latter. How the after-effect operates may be open to dispute, but that it does operate on the preceding connection seems to Professor Thorndike "as sure as the fact of learning itself" (p. 33). It will have been noticed that effect, as used, is equivalent to knowledge of progress or of results and, aside from the classification of consequences in terms of status as satisfiers and annoyers, which is wholly on the basis of behavior, is entirely without any affective implication. This treatment of effect is free from much of the criticism brought against earlier formulations of the law of effect.

The unpopularity of the doctrine of effect is laid partly to a prejudice against thinking of the effect as retroactive, and partly to a reluctance to accept it as long as its mechanism is unknown. Both should give way in face of the facts. It is first necessary to learn, however, what sorts of consequences strengthen or weaken connections, and this leads at once to a classification of many of the consequences as "satisfiers" and as "annoyers." A satisfier is "a state of affairs which the individual does nothing to avoid, often doing such things as attain and preserve it." An annoyer is "a state of affairs which the animal avoids or changes" (p. 36). The immediate question for experiment then becomes, What are the relative influences of the two consequences? The answer, obtained, for example, from learning which one of five meanings to choose for words, is that the announcement of "wrong" does not weaken the connection at all.

The "wrong" connections are, instead, displaced by the rewarded "right" ones. "Satisfiers *seem to* act more directly and generally and uniformly and subtly, but just what they do should be studied with much more care than anybody has yet devoted to it" (p. 46).

Establishment of the fact that the after-effects of a connection work back upon it calls at once for explanation. In the fourth lecture the available hypotheses are summarized and evaluated. (1) The representative or ideational theory holds that right responses are favored and wrong ones inhibited by ideas of past consequences which the situation recalls. Against it, is urged the objection that the alleged ideas do not appear often enough during learning but only in rather deliberative behavior. Further, by this theory the consequence "Wrong" should be recalled as often as the consequence "Right," yet the latter has much the greater influence. (2) According to a second hypothesis, when a satisfier results from behavior, the subject repeats the connection or its equivalent and hence gives it greater frequency. But in complicated acts there is no time to repeat, and animals can scarcely be expected to do so. Experiments, also, in which inner repetition is well excluded, support the objection. (3) A connection which is followed by a satisfier may last longer than one followed by an annoyer. This may mean "that the situation and response are held together in the mind longer," or that "the connection's physiological counterpart lasts longer" (p. 55). In its first meaning, this hypothesis may state an important accessory but not a general cause. The latter is simple but finds difficulty in explaining cases where the consequences are long delayed. (4) The last hypothesis is one advanced eighteen years ago by Professor Thorndike and which he still deems worthy of consideration "as at least above the level of phantasy." It involves the well known concept of neural readiness and places the capacity to learn and retain in "the movement-processes of the neurones." There is much more to the theory than a brief statement can convey. Its general character can, however, be indicated by two statements from the lecture. "A neurone modifies the intimacy of its synapses so as to keep intimate those by whose intimacy its other life-processes are favored." "The simple avoiding reaction of the protozoa, inherited by the neurones of the brain, is the basis of the intelligence of man. The learning of an animal is an instinct of its neurones" (p. 59). This hypothesis is, admittedly, "highly speculative, but it is not mysterious." Such processes could operate, although no important evidence can be offered that they do. In a province where hypothesis is difficult,

criticism may well be hesitant, but it seems to the reviewer a long step from the word "Right" with its ramifying meanings to the ameboid extension of a dendrite.

However unproductive theorizing about the modes in which after-effects operate may be, it is concluded from the evidence presented that the consequences act directly on the connection at the time and not indirectly by rehearsal or other roundabout procedures. The fifth lecture describes a large amount of new data upon after-effects and is offered by the lecturer, also, in refutation of doctrines "which assume that frequency, recency, congruity, facilitation of a consummatory response or of some other form of activity, or the rehearsal or revival in memory of the right response or of its after-effect, is the causal factor" (p. 65). The refutation is primarily inferential and the experimental evidence is of the same general character as that described in preceding lectures. It strongly supports the general case for effect.

The concepts of identifiability, availability, trial and system are the field of the sixth lecture. A series of experiments has studied connections in which one term is a line, geometrical figure, or complex linear shape. They illustrate the principle of "identifiability of the situation, that, other things being equal, connections are easy to form in proportion as the situation is identifiable, distinguishable from others, such that the neurones in your brain can grasp and hold and do something with it" (p. 87). Learning includes both changes in connections and in the identifiability of situations. The same principle applies, as is experimentally shown, to the availability of the response. A second set of considerations relates connections to purposive thinking and problem-solving. Connections may be from a situation to a goal by a highly variable route: and even in apparently fixed connections there is a mixture of multiple response and selection. A third set of facts concerns the tendency to make connections "subservient to certain logical and conventional systems" shown in data which are presented. The lecturer holds, however, that the simple associations and the complex systems which coöperate intimately with them in our thinking are the same in kind. For "these systems, from the humblest such as the alphabet to the proudest such as a science or a philosophy, are themselves constituted out of connections" (p. 100).

The first six lectures summarize a wealth of experimental fact, most of it new. The remaining six are devoted to a filling out of the picture sketched by the first six, to a critical examination of other

views of learning, and to a consideration of the ways in which the simpler connections earlier described operate in more complex phenomena. The experiments dealt with external states of affairs, but the principles which apply to their connections "probably apply to connections leading from thoughts or feelings to other thoughts or feelings or acts which follow and belong to the former." States of affairs within the brain may be situations as well as may external events, and the peripheralistic behaviorist "is backing the wrong horse." "A connection is no less a connection when the things connected are the subtlest relations known by man and the most elusive intellectual adjustments he can make" (p. 103). And these may go on wholly within the associative neurones of the cortex. Statements like these assume a major part of the systematic structure which is required to validate them. Subtle relations, feelings, states of affairs within the brain, and elusive intellectual adjustments can scarcely be reduced so simply to connections and the undefined terms connected. Systematic treatment of concepts such as those just mentioned, and rigorous definitions of situation and response, are sorely needed here.

The experimental results upon the ineffectiveness of sheer frequency have been cited already, together with the apparent exception offered by the conditioned response. Professor Thorndike returns now to this possible exception. The conditioned response differs from his own experiments in that mere contiguity with little belonging and no demonstrable satisfyingness causes learning after very few repetitions. The conditioned response work, however, important as it admittedly is, he interprets as a special case, not as the fundamental basis of learning. (1) The response common to the conditioned response connections in any given animal becomes very excitable and "learning to salivate at a certain signal may then be more comparable to learning by a little child to do something other than sitting still when a buzzer sounds than to learning to do any particular thing at such a sound" (p. 109). (2) The conditioned response is readily extinguished, ordinary learned connections are not. (3) This extinction is curable by disuse, ordinary connections are weakened by disuse. (4) The salivary flow is a sensitive response, yet man learns it in response to an irrelevant stimulus very slowly. In other things he is a better learner than the dog. (5) Several of the other features of the conditioned response, such as the spread of extinction effects, seem at present to have no counterparts in ordinary learning. One positive possibility is that learning may occur rapidly by mere contiguity if the stream of behavior is greatly narrowed, a condition

which does not prevail in ordinary learning. The suggestion is ventured, however, that the pure conditioned response teaches more about excitability than about learning, although the present relation of conditioned response phenomena to ordinary learning is admittedly a mystery.

In Lecture 8 we come to the relation of Thorndike's work to purposiveness and to Gestalt. In the sense that " expectations, intentions, purposes, interests, and desires refer to dynamic factors as real as the situation-response connections " (p. 120), themselves, there can be no quarrel between " honest connectionist " and " honest purposivist." Response depends upon the person as well as upon the external situation and the chief rôle is, after all, played by the learner. The quarrel, if any, will be over the account of the constitution and nature of purposes and persons. For the lecturer these are compounded out of connections. " The mind is man's connection-system. Purposes are as mechanical in their nature and action as anything else is " (p. 122).

The *Gestalt* attacks upon elementaristic theories of learning are tolerantly and fairly considered. The connectionist realizes the difficulty of an explanation of learning in terms of an addition of elements " perhaps even more clearly and acutely than the Gestaltist does. But on the whole he finds reason to think that it is being and will be so explained " (p. 125). The concepts of stress, equilibrium and other qualitative features do not seem hopeful. The *Gestalt* assertion, however, that the unit is a pattern subject to closure and *Prägnanz* is a challenge to the connectionist to produce a better principle of organization. With the *Gestalt* denial that any behavior is fully described by an enumeration of its constituents the lecturer is in sympathy. " Human behavior is not an undifferentiated series of events. Much of it falls into units longitudinally, each with a beginning and an end " (p. 126). But much of it, also, does not, and few behavior units have an analysis-defying unity and a transposability. The connectionist, moreover, has principles which render a tentatively satisfactory dynamic account of much that *Gestalt* is alleged to explain. The principles of the influence of the organization of the individual on simple connection-forming, of belonging and acceptability, of variable and selective response between situation and final goal, and the variability of " fixed " habits, point to a subtle coöperation of connections which is far from a simple elementarism. The connectionist theory may be inadequate, and its explanations of more complex processes are provisional. But Professor Thorndike " can-

not see that such a connection-system requires aid from closure or *Prägnanz.* The facts which they explain seem explainable nearly or quite as well by varied reaction guided by the satisfyingness of the results attained, and this seems far simpler and more in accord with what the neurones are and can do" (p. 131).

Thus far in the lectures no contrast has been made between learning involving overt behavior and that which involves ideas. In Lectures 9 and 10 ideational learning, thinking and reasoning are surveyed. There is more in these chapters with which the reader, familiar with Professor Thorndike's previous publications and the current of contemporary research, is acquainted, and they will be treated less in detail. An examination of what is implied in learning by ideas leads to the conclusion that "in all this there is nothing beyond or above connection-forming. Elements are made to stand out in relief and arouse response irrespective of their context by the action of use, effect, piecemeal activity, and preferential connections" (pp. 142–143). In it, as in simple associative learning, there is "control by the mind's set or disposition." A consideration, likewise, of certain elementary cases of reasoning, ends with the statement: "The compositions of forces which determine the direction of thought are thus highly elaborate and complex; but the forces themselves are very simple, being the elements in the situation and the connections leading from those elements and various combinations thereof which the past experience and present adjustment of the thinker provide" (pp. 159–160).

The two concluding lectures treat of the evolution of learning in general and in recent times. It is the thesis of the first that from the simplest organism to man mind has evolved by increase in the number and fineness of the connections which can be formed. This is the quantity theory of intellect which the lecturer has proposed elsewhere, but which is said to be in a stronger position now than when it was first published. It has the advantage of agreeing with what is known about the evolution of associative neurones and with the ontogenetic development of learning, and of being in harmony with the facts concerning the differences between individuals in the degree of development of their higher mental functions. In recent times the evolution of learning has been toward learning more and truer things and "toward learning equally difficult things more quickly and pleasantly" (p. 193). Probably man will learn more. The possibilities of his own control of the future are great.

Professor Thorndike has made in this book a major contribution

to our knowledge of the primary conditions of a simple connection-forming and has placed the facts squarely in the perspective of his quantity theory of the nature and evolution of mind. The experimental sections of the book are a digest of an implied mass of experimental data, but the reader will doubtless be willing to accept statements of fact, made by the author, in absence of full tabular proof. The criticism offered of the conditioned response and of *Gestalt* are informing, important and tolerant. He has, withal, at the points where criticism is most likely to be directed, disarmed the critic by an admission of partial knowledge and of need for further research. One feels that he appreciates the weaknesses in the structure of connectionism as thoroughly as do most of its critics and is ready to see it altered in the face of subsequent knowledge.

The systematic validity of the general theory of connections is a problem which the lectures do not explicitly treat and any discussion of it here would be misplaced. Certain things specific to the present account may be touched upon, not, however, with the suspicion that the lecturer was himself unaware of them. It is stated as a general law of mental life that response is determined by the person as well as by the situation. Nothing is offered, however, to show the specific modes of this determination, its conditions or its amount. Individuals are said to respond often in terms of systems as well as by simple reactions. Are the laws of systems constituted by connections the same as those of the individual connections? Both person and system denote a wider context-effect whereby total and complex states act on simple connections. If "the mind's set" or anything else beyond single situations, their connections, effects and the like, determine learning, the laws of simple connection-forming, however true otherwise, are incomplete to the extent that they take no more than verbal account of these wider conditions.

Consonant with these statements is the possibility that the acquisition of such relatively complex skills as maze learning, typewriting, or telegraphy may require for explanation principles which are not necessary to an understanding of simple acts of the kind studied in Thorndike's experiments. He has, however, explicitly assumed that what will explain the simple connection will explain also the most complex integration. In the absence of direct experimental evidence this is open to question. One is aware throughout that the lecturer is dealing, by means of arbitrarily simplified concepts, with a highly simplified and abstracted type of learning, in most cases several degrees more simplified than the nonsense syllable lists and formal skills of the usual laboratory studies, for these are integrations of a

relatively high order in comparison with the acts upon which the efficacy of frequency and effect have been tested. Science must work with abstract and simplified materials, but it must also validate its claim that what holds for such materials holds for more complex relationships.

At many points factors coöperating with frequency and effect are mentioned and dismissed without further treatment. In experiments which compare punishment and reward, "the differential must be considered" (p. 38). A part of a situation may evoke the whole response (p. 139), but the complexities of redintegration are given little notice. Meanings, which are connections, may be recognized as right or wrong, may satisfy or fail to satisfy the prevailing mental set. But here recognition and satisfaction seem to take on a meaning which is more than a synonymity with connection. Moreover, at times meanings and neurones, perceived "Rights" and extending dendrites, are mingled in a way which dazzlingly outruns neurophysiology.

The book is strongest on the side of direct experiment upon simple connections. It is less strong when it comes to the so-called higher processes, and to theoretical explanation as in the case of the action of effect. Criticism, however, is easy, and the greatest experimental and theoretical constructions are open at some points to question. Adverse criticism is frankly feeble against the tremendous body of fact which the book represents. Professor Thorndike has made a direct and admirable attack upon a series of major problems. It matters little that a particular reviewer may demand more detailed theoretical analysis. The experimental facts stand beyond cavil and upon such facts the structure of science is built. Were it the custom for writers of books to make, in the space of 200 small pages, a major contribution to experimental fact, infused with tolerance and maturity of interpretation, psychology, of whatever theoretical stripe, would be infinitely richer. The book is of an order amply to support the closing sentences of the last lecture, which epitomize learning's course and goal. "It is a noble thing that human reason, bred of a myriad unreasoned happenings, and driven forth into life by whips made aeons ago with no thought of man's higher wants, can yet turn back to understand man's birth, survey his journey, chart and steer his future course, and free him from barriers without and defects within. Until the last removable impediment in man's own nature dies childless, human reason will not rest."

John A. McGeoch

University of Missouri

Dodge, Raymond. *Conditions and Consequences of Human Variability.* New Haven: Yale University Press, 1931. Pp. xi+162.

Those who have studied Dodge's first accounts of his various researches will find this book a convenient and interesting review of many of them. Those who have not will be stimulated to do so, especially to learn about the extraordinarily ingenious techniques for measuring and recording human responses which enabled him to make his discoveries. Both groups will welcome the statements concerning the general problems of variability, including modifiability, and the significance of experimental results in relation to them.

The book presents facts concerning refractory phase, relative fatigue, inhibition, summation, the simpler forms of organizations or patterns, and the more elaborate organizations or systems, up to and including those in which conceivably every past event in the person's mental history may share in determining his present response. Especially important are the demonstrations that periods of unreadiness to act and differences in their length may have consequences reaching beyond the temporary restriction of action in the unit in question, that the competition among possible responses to a situation may be decided by a very small difference which then has very large consequences, that stimuli too weak to cause an overt positive response may yet produce some of the characteristic results of such positive action, and that even so simple patterns or accomplishments as oscillating a finger up and down or following the regular swing of a pendulum with the eyes involve learning, selection, and a rather elaborate system of checks and balances.

The general comments cover a wide range, touching on the rôles of the refractory period and of relative fatigue in mental life, the co-action of "large" organizations such as *Gestalten* with "small" organizations such as single situation-response connections, the influence of past experience upon present responses, *Gestalt* theory, behaviorism, consciousness, psychophysical parallelism, and even theology.

I shall not mar the reader's enjoyment by making a summary of these. Three quotations will give some idea of their scope and spirit. Of the possible range of influence of the refractory phase, Dodge writes:

"With a little speculative license one might conjecture what effect differential barriers to repetition would have on ideational contents consequent to repeated experiences. One might ask what would be the result if there were greater barriers to the repetition of specific

concrete ideas than to the repetition of general ideas. The latter would obviously tend to survive and dominate consciousness while the former would tend to disappear. It is conceivable that even without direct reinforcement a single idea of very short refractory phase might survive indefinitely in consciousness. It seems probable that some analogous selective process is at work participating in the control of both intellectual content and habitual acts. Not only is the barrier against repetition of the beautiful and the general apparently less than that against the ugly and the specific, but certain moral and ethical ideas with both these factors in their favor might persist for long periods of time."

Against the general doctrine by which we were taught to view mental facts and physical facts a generation ago he protests:

"Further evidence for the inadequacy of parallelism as a working hypothesis for inner psychophysics is the fact that it breaks down when we really use it, and leads to absurdity. We are utterly unable to reason successfully either from known nervous facts to consciousness, or from consciousness to its nervous correlates. Phenomenal parallelism as a working hypothesis for inner psychophysics assumes too much. It is equally embarrassed by the question where in the scale of organic existence a consciousness comparable to our own begins, as well as by the question concerning the specific conditions of the only consciousness which we can know directly. . . . If it is retained at all, the hypothesis of psychophysical parallelism must be tinkered with before it will fit the facts. It cannot be trusted anywhere, and is consequently a debatable theory rather than a serviceable working hypothesis."

Dodge's own synthetic solution to the problem of mind and brain is put in the form of three questions:

"1. If we postulate a living tissue of such character: (1) that n changes in its environment (nS) tend to excite n characteristically different reactive modifications in the life history of its elements and their organic interrelations (nR); (2) that every new S tends to reproduce every previous R in definite sequences, though in various degrees of completeness; (3) so that every new R becomes a part of a relatively slowly changing system of reactive modifications, with which it thus becomes organically integrated and to which it adds its peculiarity, should we not, with such a reproductive organization of its reactive modifications, grant to our tissue a kind of intellective consciousness, in which the qualities of the original nR are logical accidents, depending on the number of discrete reactive modifications

of which the elements of our hypothetical tissue are capable within the limits of their organic integration?

"2. If we further postulate a circular excito-reactive process by which the directly excited factors of the system tend to reinforce the S which aroused them, and to emphasize the corresponding R, together with other R's immediately connected with it, while it tends to inhibit less directly connected processes, within the limits of relative fatigue, would we not therewith grant our tissue a kind of selective attention?

"3. If we further postulate a relatively slowly changing group of emphasized R's, which are regularly reproduced with every new R, must we not therewith grant our tissue a kind of personality?"

The writer is unable to wax enthusiastic about Dodge's theory of consciousness—or anybody else's! I am willing to answer *Yes* to his questions, but it seems probable that I should go on with my psychological work in just the same way if I answered *No*. I doubt whether it has been of much use to him in his work. An impartial observer would "conjecture" (as Dodge is fond of saying unless he has the fact actually photographed and chronoscoped) that satisfactory work in psychology can be done by introspectionists and behaviorists, by Gestaltists and connectionists, by psychiatrists, psychologists, anthropologists, and statisticians. I would conjecture that Darwin and Galton would have given definitions of consciousness which would be marked C or lower in our college courses in psychology. However, if Dodge's work is furthered by his doctrine of consciousness or by Erdman's doctrine of "non-independent reproduction by apperceptive fusion," then I believe in both of them—for him.

All psychologists will hope that he will push that work, especially the isolation of units of behavior in the intact organism and the accurate recording of their essential features, on to the study of other and "higher" units, and that he will extend its applications among the lower animals. The new methods of observation by the action currents should be a powerful tool in his expert hands.

Edward L. Thorndike

Teachers College, Columbia University

Laird, Donald A., and Muller, Chas. G. *Sleep: Why We Need It and How to Get It.* New York: John Day Co., 1930. Pp. xi+214.

Although this book is written in the style of a Sunday magazine writer, its authors say that "it contains a great number of new

experimentally verified and discovered facts about sleep," on which some fifteen people, most of whom were undergraduate students, have been working " for six years "—or at least *within* those limits of time. It receives a critical review in this journal because it brings some of these alleged facts into light for the first time. Since the book was reported among the best sellers in the class of non-fiction for a period of some weeks, one would judge that it has attracted and perhaps entertained a good many general readers. In so doing, however, it has presented a conception of fact that is about as accurate as the representations of a modern " health food " advertisement, or an old-fashioned patent medicine booklet. We shall consider some of these extravagances.

Fatigue is accounted for in the following simple fashion: If a muscle is overworked, lactic acid forms more rapidly than it can be " removed." It presently appears that " removal " here means recombination of part of the lactic acid into glycogen (or glucose) with oxidation of the remaining part. As long as an excess of lactic acid remains in the muscle that was exercised, it will interfere with the performance of the latter. This condition may be relieved in the over-exercised muscle by the transportation (*via* the blood-stream) of the excess of lactic acid to other parts of the body, and more specifically, to muscles that have not been overworked, in which there is a greater supply of available oxygen.

While the overworked muscle is benefited by the transfer, the muscles into which the excess is imported are thereby " made tired." Thus, exercising an arm may fatigue the whole body; while on the other hand, an unfatigued arm-muscle may help an overworked leg-muscle to recover, etc., etc.[1]

It is true that respectable authority can be found for each of these

[1] In this chapter, the authors " give verisimilitude to a bald and unconvincing narrative " by presenting " Stonewall " Jackson in the novel rôle of Efficiency Expert. They say that this " old Confederate general "—who died at the age of thirty-nine—prolonged the Civil War two years " when he marched his troops with unexpected speed across the Potomac from Washington "—where they never were—" in defense of Richmond." His actually remarkable feats of moving half-disciplined infantry quickly to the place where they were most needed, they explain by telling us that he scientifically coddled his men, requiring them to punctuate their marching by brief rest-pauses (a very old military practice, by the way), and by permitting their strength to be restored " by sugar and other carbohydrates from the sutlers' wagons."

These are fancies. According to Jackson's biographers (2, 3, 11) he employed a far simpler method. He made heavier exactions of his troops in emergencies than popularity-seeking commanders dared to make; and then

assertions. However, it is well to examine the facts—actual and potential—on which such an argument might be based. Obviously, the first question is whether, following the exercise of an arm-muscle, for example, one can reliably detect an excess of lactic acid (or of any of its salts) *inside the boundary-membranes* of a remote muscle, such as in one of the legs. (To find lactates in the bodily fluids that bathe the outside of these membranes would not serve to establish the doctrine.) But even if this excess were to be found in the

browbeat his subordinate commanders into seeing that those demands were met. In times of stress, he scorned the most elementary rules of dietetics. Some of his heaviest marches were made *without rations* (2, 277; 3, 388). When it appeared to be necessary he might leave his commissary wagons behind for five or six days at a time rather than allow them to lengthen and retard his columns (2, 304; 3, 516); it is very unlikely, therefore, that he allowed sutlers and other camp-followers to encumber a forced march. It is said (11, 117) that his infantry usually marched without knapsacks, eating a three-day ration as soon as they could cook it, and going for the next two or more days without food, except for green corn, apples, *etc.*, which they might take from the fields that they passed. Dysentery of course resulted from this practice, and rendered many of his men ineffective.

Nor did Jackson pay more regard in emergencies to the rules of rest. In his retreat from Harper's Ferry, in the spring of 1862, he ordered the rear-most brigade not to pause until it had passed Winchester, over thirty miles away (3, 388). In his march upon Manassas Junction, a few months later, he is said to have ordered twenty minutes of marching and two minutes of rest, lying down (11, 201), which is certainly not a great deal; while in the invasion of Maryland, immediately thereafter, he seems to have ordered five miles of marching and five minutes of rest (12)—hardly more time than is demanded for excretion. This march, however, was disorderly. Nearly half of the Confederate forces dropped out before the main body reached Frederick—the majority being exhausted or too weak to keep up. Those who presently fought at Sharpsburg, or Antietam, were "ragged and filthy, . . . sick, hungry, and in all ways miserable" (2, 332). Jackson himself was so nearly worn out that he twice fell asleep during the very brief call which the Federal commander at Harper's Ferry made upon him to offer the surrender of that garrison (2, 325).

On the whole the descriptions of Jackson's tactics which one finds in the military histories and biographies do not suggest "efficiency" in the sense of a given achievement obtained with relatively little cost; but of great achievement purchased by great expenditure of energy; so that the formulas used by the Efficiency Expert might require one to say that his performances were very inefficient, indeed.

We have discussed this point at length, partly because the authors' treatment seems to illustrate their general regard for accuracy; but partly because we feel that "Stonewall" Jackson's actual merits were great enough for any one hero, and that the political orators, preachers and novelists have already invented for him rather more imaginary virtues than a single character can be made to bear.

interior, it would not do to attribute it to importation from the overworked arm without first disposing of some alternatives. One of them is this: that when the arm was active, other muscles of the body, including those of the leg, became active, tense and strained "in sympathy" therewith, and while doing so, generated a little excess of lactic acid by their own participation in the general activity. Such diffusion of muscular response appears to be the rule rather than the exception; and there is reason for believing that it aids in maintaining specific activity—possibly by serving to generate additional nervous energy to impose on the failing musculature (4, 372 ff; 5, 10).

On the whole, the alleged finding and its explanation seem to us to be far too simple. A factory employee may work for eight or nine hours at a single task. A restricted set of musculature is overworked, and has become feeble and clumsy—*in meeting that particular demand.* But he may now "rest" himself, by indulging in an hour or so of athletics. In this hour of play, he may consume more bodily fuel and generate more lactic acid than in an hour devoted to his factory work. In his play, he is using a comparatively large portion of his total musculature, including a good part of that which was exercised in his occupation; only, it is now undergoing a new pattern and succession of strains. By hypothesis, all this musculature is fatigued—part of it by physical exercise, and the remainder by importation of lactates. Nevertheless, when he ends the game, he may be more capable of meeting the demands of the present than he was at the beginning. If these are facts, they prohibit one from identifying post-exertional feebleness and clumsiness with concentration of lactates, or from supposing (without evidence) that they are measured thereby. While lactates are indeed products of muscular exertion, so that high concentrations thereof may ordinarily indicate that muscular work has recently been done, they are not necessarily the cause of "fatigue," or even an important agent in its production.

On page 57, the authors say that "physical fatigue is healthy fatigue. The body needs such stimulation, and fatigue from physical work seldom is dangerous. Fatigue caused by mental work is harmful *because it usually involves no physical activity* with accompanying stimulation of circulation, and must therefore be carefully handled for recuperation."

The italics in this quotation are ours, and were used to exhibit an assertion whose meaning we cannot make out. On its face it might

be taken to mean that "mental work" involves no transformation of energy; but that interpretation would contradict the claim made on pages 19 ff. that the experimenters at Colgate are in command of a means of measuring the energy that is expended in the "mental" work of multiplying. This claim, which had been made previously (**8**), was recently subjected to a critical review (**7**), and was shown to rest upon a fallacy known as equivocation. It is repeated in this book, however, and made the basis of still further pronouncements.

What the experimenters did, according to their own accounts, was this: they collected a sample of the air which a subject exhaled during five minutes, after he awoke in the morning and before he left the bed. Analysis showed how much oxygen the subject had removed from the sample; and by calculation, the quantity of heat, in calories, that he had generated could be derived. Multiplying this value by twelve (since it was given off in one-twelfth hour) gave the calories generated per hour at the rate that was indicated for this five-minute interval. The subject was then handed a typewritten list of problems to be solved by "mental" multiplication, and after ten minutes of such work, while he was still multiplying and still lying in the same position in bed—an unusual thing for a waking person to do—the experimenters collected another five-minutes' exhalation, and subjected it to the same treatment. It seems that the second sample always contained less oxygen and more carbon dioxide than the first: whence the experimenters should have said that the subject, *while* he was multiplying, consumed more bodily fuels than while he was not. What they did say, however, is that he consumed more energy *in* multiplying than when he refrained therefrom, and this is quite a different matter. For merely requiring him to multiply may also set up increased muscular tension all over the body, with tendencies, at least, to gesticulate and fidget; and these activities may stimulate a variety of organic activities, as Benedict and Benedict (**1**) have indeed reported. How much of the increased energy-expenditure should be ascribed to these activities, and how much to the *act* of "mental" multiplication? There is no possible means of deciding, because the physiologist and biophysicist have no means of segregating these activities for separate measurement. This much is certain: that if one attributes all the expenditure to the "mental" work alone, his estimate will be too high. The more highly skilled the subject becomes in multiplying while remaining relaxed, the smaller that estimate will become.

Now the authors of this book report rather large increases—from 9 to 35 per cent above the expenditure found during "idleness"—which should be compared with the values recently reported by Benedict and Benedict (1) to the National Academy of Sciences. (It should be borne in mind that the senior author of the latter study has been specializing in measurements of metabolism, by the most refined methods, for many years, and is usually considered an authority.) These investigators, who also required their subjects to multiply, used a working period of an hour or more, during which determinations were made continuously, instead of through periods of five minutes. The increased consumption which they found is relatively much less than that which the authors of this book report, and is absolutely very small. They say that "the extra calories needed for one hour of intense mental effort would be completely met by the eating of one oyster cracker or one-half of a salted peanut." On such a basis, it would appear to be a trivial matter if the energy demanded for mental work were quartered or quadrupled.

The authors of this book, however, report relatively large demands: *e.g.*, about 9 per cent of the resting consumption if the subject sleeps eight hours, and about 27 per cent if he sleeps only six hours (p. 21) for about the same output. Also (p. 149) about 26 per cent of the resting consumption if the subject has slept on an upright-coil spring, and about 32 per cent if he has slept on a link spring suspended at the two ends. Furthermore (p. 153) about 18 per cent if the subject uses a coil spring that is of "medium" stiffness, 31 per cent if it is "almost as hard as an ironing board"; and about 35 per cent if it is "very soft."

Taking the last comparison: if the subject uses the "medium" spring, it is represented that a given amount of "mental work" "requires" about half the expenditure of energy that is required if he uses the "very soft" spring. According to Laird and Wheeler's concept of "mental efficiency in the true sense" (**8**), this means that he is mentally twice as efficient if he uses the "medium" spring as he would be if he used the "very soft" spring. If, however, he should replace the "soft" spring with the "medium" spring, expecting to solve twice as many problems, or to think out twice as many ideas, or convince twice as many people that they should buy his product, on the same expenditure of energy, he would in all probability be disappointed; for there is no evidence whatever that the energy that he might thus save, whatever its magnitude may be, is thereby made available for more "mental work." However, it is by

such an interpretation that the finding acquires human interest, for it is an increased *output* that the seeker for "efficiency" is seeking; he is not greatly concerned with energy-costs.

The authors present a third claim which has importance, and which it is our duty to examine. They assert that they have made measurements of "muscular tension" and that they find it greater at some times of the night than at other times; and that it is markedly increased by noises (p. 101) such as those of passing trucks, slamming windows, and the cough of another sleeper. It is also represented (p. 137) that "the slight noise made by walking past a sleeper's bed on tiptoe increased the sleeper's muscle tension almost to waking, although the sleeper did not wake, and after getting up, did not know that anyone had been near him during the night. That the bodily changes they cause last for as much as half an hour after noises have ceased, is a pernicious feature. . . ." Moreover, a "hammock spring" (p. 151) is said to keep muscles "tensed all night long"; while too thick a pillow (p. 163) has a similar effect.

All this is interesting, if true. The only trouble that we can find in the authors' assertions is that they do not rest upon measurements of muscular relaxation and tension, at all. There is, in fact, no feasible method of measuring the tension of muscle in an intact organism. Neither could one conclude, from the measurements of the tension in one or a few muscles, what the average tension is in the whole system of musculature. Our own photographic studies show very clearly that in any sleeping posture, some musculature is kept under continued strain, to maintain the pose; while it works, other portions may rest (hence the need for frequent shifts from one pose to another), but seemingly the whole system is never relaxed at once.

What the authors have done is to measure the resistance which some portions of the body offer to an applied electromotive force: they have called these measurements expressions of muscular tension. Now it is a fact that changes in the reading of the galvanometer sometimes occur when the subject tenses a set of muscles; but there are numerous other physical changes that produce an identical effect. Among them are the rate at which sweat is excreted, changes in circulation in the skin, relaxation and constriction of the pores of the sweat-glands, the area of contact and the pressure exerted between electrode and skin, physical and chemical changes in the electrodes themselves, and a variety of other factors that have not yet been analyzed. The work of Landis (9), while open to criticism, showed

that changes in bodily resistance are not definitely associated with sleep and waking, with the sleeper's stirring while awake, or with "mental activity" as expressed by conversation, during the waking periods. Until more is known about the phenomenon than anyone knows at the present time, it is preposterous to attribute it to any one kind of bodily change—such as changes in tension—without showing *by independent means* that the latter change, and no other, has accompanied the change in resistance. This the authors have not done, and cannot do. Meanwhile, their interpretation is not only unwarranted, but deceitful, for the popular reader, to whom the book is addressed, cannot judge for himself, and is given no warning that the evidence contains any possible flaws.

Various devices are suggested for obtaining sleep. The bedroom, they say, should be painted blue or green, "for, though we sleep in the dark, we sleep better and more quickly recoup from fatigue when the bedroom is intelligently lighted." Light, they say, should be excluded. This recommendation would be weakened in the eyes of some readers if they could see how quickly some people learn to sleep soundly in the glare of overhead lamps, or lying naked and fully exposed to the sun. It might be affected even by a few minutes devoted to observation of the day-time sleep of domestic animals, such as the horse, the dog, or the cat. The authors also recommend that noises be suppressed or excluded by special building materials—overlooking the fact that rest-cures are often sought at the most noisy of all places—namely the seaside. Recommendations are made for special beds, coverings, pajamas, perfumes, beverages, and postures, with little or no regard to the commonsense observation that by self-training, a person can learn to sleep at almost any time or place or under almost any set of conditions, if he must.

The book has been enthusiastically reviewed in certain trade journals, because of the abundance of talking points in favor of these products which it renders available to the salesmen "contacting the consuming trade."

H. M. Johnson
G. E. Weigand

Simmons Investigation of Sleep
Mellon Institute of Industrial Research
University of Pittsburgh

REFERENCES

1. Benedict, F. E., and Benedict, C., The Energy Requirements of Intense Mental Effort. (Abstract.) *Science*, 1930, 71, 567.
2. Cooke, J. E., *Stonewall Jackson: A Military Biography*. New York: D. Appleton and Co., 1876.

3. DABNEY, MAJOR R. L. (C.S.A.), *Life and Campaigns of Lieut. Gen. T. J. Jackson* (Stonewall Jackson). New York: Blelock and Co., 1866.
4. JAMES, W., *Principles of Psychology,* Vol. 2. New York: Henry Holt and Co., 1890, Ch. Dynamogenesis, pp. 373 ff.
5. JOHNSON, H. M., The Definition and Measurement of Attention. *Am. Journ. Psychol.,* 1925, **36**, 601–614.
6. JOHNSON, H. M., Noise: A Social Problem. *Harper's Magazine,* **159**, 561–571.
7. JOHNSON, H. M., and SWAN, T. H., Sleep. *Psychol. Bull.,* 1930, **27**, 1–39.
8. LAIRD, D. A., and WHEELER, W., JR., What It Costs to Lose Sleep. *Indus. Psychol.,* 1926, **1**, 694–696.
9. LANDIS, C., Electrical Phenomena of the Body During Sleep. *Am. Journ. Physiol.,* 1927, **81**, 6–17.
10. PILLSBURY, W. B., *Attention.* New York: Macmillan, 1908.
11. TATE, ALLEN, *Stonewall Jackson.* New York: Minton, Balch & Co., 1928.
12. YOUNG, J. C., *Marse Robert.* New York: R. D. Henkle Co., 1929.

TERMAN, L. M. *Genetic Studies of Genius: III. The Promise of Youth.* B. S. Burks, D. W. Jensen, L. M. Terman. Stanford University Press, 1930. 508 pp.

This is a series of follow-up studies of a thousand gifted children whose mental and physical traits were first reported in Volume I of Genetic Studies in Genius. The purpose of the study, in the language of the authors, is "(1) to serve as a check upon the correctness of the conclusions presented in Volume I; (2) by obtaining various kinds of data not obtained in the earlier investigation to help complete the picture of the typical gifted youth."

The authors achieved remarkable success in re-locating the original subjects and in securing their coöperation. The interval between the original location of the gifted children and the writing of this volume is approximately eight years. In the main experimental group of 1924 there were 643 cases of whom 587 coöperated in the follow-up. An equally good proportion of the "outside cases" and their siblings were re-located. This fact in and of itself is of great significance in showing that such follow-up studies are indeed possible and within the limits of scientific procedures.

The following tests were given to the main experimental group in 1927–28: (1) Stanford-Binet for all below ninth grade, and the Terman Group Ability for all others. (2) Stanford Achievement for all in grades four to eight; Hotz Algebra in grades nine, ten, and eleven; Stanford Literary Comprehension in grades nine, ten, and eleven; Iowa High School Content Examination for all high school seniors and college freshmen. (3) The Woodworth-Cady Test of

emotional stability in grades four to eight; the Wyman Interest Test for all who took it in 1922; the Stanford Masculinity-Femininity Test for Los Angeles subjects in grades nine to twelve; the George Washington Social Intelligence Test for high school seniors and college freshmen; the Watson Test of Social Attitudes for high school seniors and college freshmen; and the Strong Occupational Interest Test for boys of the regular group over fifteen years of age.

In addition to these intelligence, achievement, and personality tests, nine other kinds of information were secured including a Home Information Blank, Interest Blank, Trait Rating Blank, Report of Interview with child, parents, and teachers.

It should be noted that most of the tests given in 1927–28 are not repeats or retests of the test administered in 1921–22. There are two reasons for this: (1) a great many of the tests used in 1921–22 were not suitable for retesting purposes because they do not cover the necessary age span; (2) the authors are not only interested in changes in individual growth and achievement, but also in whether or not the general cross-section picture of 1927–28 of the gifted subject is the same as that of 1921–22.

The Stanford-Binet Test was repeated on all of the follow-ups under thirteen years and 0 months at the time of the 1927–28 study. In this group there were 38 boys and 35 girls. In 1921–22 these boys had an average I.Q. of 146, and in 1927–28 it dropped to 143, a decrease of 3 points. The 35 girls averaged 149 in 1921–22, and 136 in 1927–28, an average drop of 13 points. There were wide individual differences in changes in I.Q. While the average was decreased yet there were individuals whose I.Q.s increased. For example, 8 boys lost as much as 15 points and 7 gained as much as 15 points; 16 girls lost as much as 15 points and one gained 15 points. These changes in I.Q. of gifted subjects are estimated to be about the same as would be expected in a normal population when the correlation between test and retest is about .75 to .80. Retests with the Terman Group Ability Test show the same general average decrease and the same general fluctuation.

The authors are at somewhat of a loss to understand this drop in I.Q. and the tendency for the I.Q. to fluctuate. In the first place they checked on the reliability of the test records. The extreme cases were tested with the Herring Revision of the Binet. The authors are of the opinion that the Herring Test confirms the Binet findings even though the figures are far from agreement. They

interpret the difference to be due to differences in the standardization of the tests. Assuming that their I.Q. figures have thus been demonstrated to be reliable they check through a list of possible causes seeking an explanation for these changes in I.Q., especially the average drop. The variables checked are health, race, certain personality factors, and certain environmental factors measured by occupation of father. In none of these do they find significant differences between those whose I.Q.s increased and those whose I.Q.s decreased.

Since these observed changes in I.Q. cannot be accounted for by any visible data of nurture, they are probably therefore attributable to nature. Thus reason the authors. They arrive at what they call "a change-of-rate-theory which is reasonable and does no violence to established facts." There are at least two objections to this theory. (1) The possible environmental or nurture factors were not sufficiently canvassed. The authors assert that since nothing showed up in the gross analysis, nothing is likely to appear on a finer analysis. The very language they use in this connection indicates that they tend to deprecate other possible nurture factors. A sample of the language is "mysterious subterranean influences." Any one who will run through their measures of health, personality, and environment will be convinced that even the grossest of the nurture features have not been covered. Evidently the well known Stanford hereditary bias is here asserting itself. One wonders why the authors did not compare the gainers and losers in I.Q. in respect to (a) per cent of school attendance, (b) attitude toward school work, (c) interests, and especially changes in interests and, (d) hours spent in study. (2) Hitherto the constancy of the I.Q. has been regarded as one of the strongest arguments in favor of heredity in intellect. Now it appears that the inconstancy of the I.Q. is interpreted as evidence of heredity. How can both the constancy and the inconstancy of the I.Q. be used to argue the same point? The answer probably is that some individuals are endowed with a constant I.Q.; others with an inconstant I.Q. If this is true then there is no possibility of ever getting further light on the question with retest data.

In addition to the changes just noted in I.Q. the gifted follow-up group shows the following outstanding characteristics: their mean educational quotient measured by the Stanford Achievement Test is somewhat lower (6 E.Q. points for boys and 10 E.Q. points for

girls) than a similar group of the same age who took this test in 1922. In all other achievement tests, such as Hotz Algebra, Iowa High School Content, there is a tendency for the gifted to rate above the control group. The high school and college grades of the gifted groups are uniformly higher than control groups, the gifted high school students receive " A " grades from four to eight times as frequently as unselected pupils, and gifted college students receive Phi Beta Kappa about twice as frequently in proportion to their number as the total college population. Thus in school achievement they rank well above the school populations of which they are a part.

In personality traits and interests the gifted are in most instances well up to such standards of normality as are available. None of the personality or interests tests showed them to be in the least peculiar, queer, or freakish. In point of health they are also normal or above. In respect to intellectual status and academic achievement the above results are fairly clean-cut. The main difficulty is that there are so few tests suitable for this sort of longitudinal study. There are very few tests that are equally applicable in the third and twelfth grades. The authors point out that the eight year interval for follow-up is nearly ideal. It seems to the reviewer that it is too long for most tests. The average age span of most intelligence and achievement tests, with the exception of the Stanford-Binet, is not over five years. This is notably true of achievement tests.

In respect to character and personality traits including emotional stability, social intelligence, interests, attitudes, and a list of behavior traits, also in respect to health and social environment, the data are naturally crude and the results more ambiguous. Here one feels the need of more precise techniques of measurement and especially the need of standards.

Part II of the volume consists of one hundred pages of case studies. Ever since 1920 Professor Terman has been keeping an active file of cases of gifted children. Some of them have had very interesting careers. From these he has selected certain ones for the purpose of illustrating such factors as special abilities, marked deterioration in I.Q. and personality difficulties.

Part III is a doctor's dissertation by Miss Jensen on a Tentative Scale for Rating Literary Juvenilia.

In Part IV the general conclusions of the report are summed up.

This volume might be criticized adversely by pointing to the small proportion of space allotted to the statistical findings and the

large proportion to case histories and the Scale for Literary Juvenilia. But the reviewer is not disposed to do so. The case studies are well chosen as illustrations of important truths which cannot be stated quantitatively. Furthermore they help make the total study valuable to readers who dislike statistics.

On the whole this report represents a type of research that is greatly needed in the educational world. Fully 90 per cent of the educational research has been of the cross-sectional variety. Very few investigators have the funds for the pursuit of the long-termed longitudinal study. Wherever the genetic method has been used and used wisely the results have been worth while. And while this third volume is not quite as spectacular as the first, yet it is probably far more significant. If it does no more than to set the pace for further studies of a similar nature it will have been worth the effort.

Its practical educational value is also great. When parents and zealous relatives are finally convinced that precocious children are no more likely to be feminine, queer, weakly, or smartalecky, than any normal child, the opportunities for full educational achievement of budding geniuses is much greater. If schools, parents, doctors, ministers, and aunts, do no more than not to interfere with the educational development of gifted children their chances of achieving greatness may be immensely enhanced. The fact that gifted girls appear to lose ground more frequently and more rapidly than boys may be accounted for by differences in the way they are treated by parents, teachers and others. This seems to the reviewer a much more likely explanation than any of the *four* offered by the authors. This suggests again that the subtle forces of social environment which are neither " mysterious " nor " subterranean " may play a far more important rôle in the development of gifted children than the authors are disposed to admit.

Mark A. May

Yale University

Holzinger, Karl J. *Statistical Resumé of the Spearman Two-Factor Theory*. Chicago: University of Chicago Press, 1930. Pp. 43.

Students of quantitative method in psychology will be grateful to Professor Holzinger for gathering into a monograph of 43 pages the statistical framework of Spearman's well known Two-Factor Theory. During the past 25 years many valuable researches dealing

with the location of abilities, their organization and relationships, have come from Spearman's laboratory. Such a large amount of work in one field has inevitably entailed some revamping and extension of original hypotheses with the result that the present-day student finds it difficult to get a clear idea of just what are the essential statistical techniques underlying Spearman's theory. To be sure, Spearman has recently (1927) summarized much of his statistics in the Appendix of his *Abilities of Man.* But the treatment here is necessarily much condensed, and this fact plus several unfortunate misprints in important formulas, makes Holzinger's clearly written review especially timely.

Holzinger styles his study a resumé, but it is actually more than this. In addition to Spearman's theory, some of the recent work of T. L. Kelley and others is related to the main topic, while Holzinger has undertaken at several points to defend and justify Spearman's various techniques. Because of this, any criticism of Holzinger's paper—while indirectly, of course, a criticism of Spearman—is properly directed toward the author.

Probably the first thing to strike a careful reader of this report is the equivocal use of the term "g" or *g* as Spearman writes it. Both Holzinger and Spearman have been guilty of this. Even critics of Spearman's theory will admit that when the three tetrad equations resulting from four tests equal zero (within their PEs) the intercorrelations may be represented as arising from one general factor which runs through all four tests, plus in addition thereto other factors specific to the individual tests. But Spearman's *g* is not simply *a* general factor, but *the* general factor which he conceives to be present in all cognitive activities. It is a very different thing, surely, to use the term "g" simply to denote *any* factor general to a given group of tests, and to use it to denote an entity—general energy or whatnot—which is everywhere and always the same. Such confusion of terms is certainly troublesome, and especially so when it seems quite clear that the factor general to a group of tests is almost certainly *not* Spearman's *g*. Perhaps this is nowhere to be seen more clearly than in Holzinger's example (page 12) given to illustrate Spearman's basic theorem. Data were taken from an experiment of T. L. Kelley (*Crossroads in the Mind of Man,* page 100) in which the population consisted of 140 Seventh Grade children. We may follow Holzinger's nomenclature as follows:

Let x_1 = reading speed
x_2 = arithmetic power
x_3 = memory for words
x_4 = memory for meaningless symbols
x_5 = memory for meaningful symbols

Intercorrelations were as shown below:

	1	2	3	4	5
1	..	.059	.195	.200	.297
2		..	.149	.284	.249
3			..	.466	.669
4				..	.692

From these five tests 15 tetrad differences are obtainable, all of which were calculated by Holzinger together with their PEs. The largest tetrad difference was —.087 ±.040, which, though twice its PE, is probably not significant as the remaining 14 tetrad differences are all insignificant when evaluated in terms of their respective PEs. It seems clear, therefore, that these five test abilities may be fairly represented by a single general factor plus specific factors. Is this central factor *g* in Spearman's sense? The tetrad differences, of course, can give no answer to this question, but a clue, at least, as to the probable nature of this general factor may be obtained by examining the correlation of each test with the factor central to them all. These correlations are given by Holzinger (page 18) as follows: $r_{1g} = .280$; $r_{2g} = .275$; $r_{3g} = .641$; $r_{4g} = .762$; $r_{5g} = .973$. Basing his conclusion upon the satisfaction of the tetrad criterion and these *rs* with the central factor, Holzinger decides that the central factor is Spearman's *g*. He writes further " . . . the so-called 'memory tests' appear to be most highly saturated with *g*, but an examination of these tests shows that they are of the intelligence test variety rather than pure measures of retentivity which has a low correlation with *g*."

This interpretation, it seems to the reviewer, is almost certainly erroneous, and is due largely to the notion that *any* general factor is at once identifiable with Spearman's *g*. It is difficult to justify Holzinger's remark that the memory tests are of the "intelligence variety rather than pure measures of retentivity." Not only were these tests designed by Kelley specifically to measure retentivity, but they have much higher correlations *inter se* than with the other tests of reading, arithmetic and speed as an examination of Kelley's Table X will reveal. All of these tests of memory have high cor-

relations with their central factor, while (1) reading speed, and (2) arithmetic power have low correlations with the same factor. If we compute the percentage of the variance (σ^2) of each test which can be explained as dependent upon the central factor, and the percentage dependent upon the factors specific to each test, we get the following interesting result:

	Percentage of Variance due to	
	General Factor	*Specific Factors*
Test 1	8	92
" 2	8	92
" 3	41	59
" 4	58	42
" 5	95	5

From the table it seems evident that the central factor is almost certainly a memory factor and not *g* in Spearman's sense of general ability or general intelligence. Instead of the memory tests being highly saturated with *g*, it would seem more accurate to say that the reading and arithmetic tests are slightly saturated with memory, 92 per cent of their variances being due to specific factors. Spearman's criterion for establishing the existence of *a* general factor is undeniably a valuable technique, but the interpretation of just what constitutes this general factor would seem to be not so much a matter of mathematical as of psychological analysis, plus, perhaps, a bit of that ever helpful trait called common sense.

Several minor points of method may be noted in passing. Spearman's formula for predicting the *g* of an individual is practically of little if any value since the *r* of the independent variable or variables with *g* must be practically 1.00 in order to reduce the PE of estimate to a point where *g* can be more than very approximately located. Correlations around .99 are rarely if ever found in practice, even with large batteries of tests. Again the partial correlation formula for finding the *r* between the specific factors in two tests (page 25) must be used with caution, since it involves the correlations r_{ag} and r_{bg}, the values of which depend upon $r_{sa.sb}$ being zero. Holzinger mentions this fact, but the point might be further stressed.

It is hard to follow the logic of Professor Holzinger's argument that the unreliability of tests has no influence upon the tetrad difference. If the tetrad equation is zero, this is, of course, true. But if, as generally happens, the tetrad difference is numerically greater than zero, the unreliability of the tests must inevitably affect the

criterion adversely. To take a concrete example, suppose that we have four tests, 1, 2, 3, 4, in which the intercorrelations are as follows:

$$\begin{array}{lll} r_{12} = .60 & r_{23} = .20 & r_{34} = .40 \\ r_{13} = .50 & r_{24} = .30 & \\ r_{14} = .40 & & \end{array}$$

Let the population be 144 and the reliabilities of the tests be $r_{11} = .80$; $r_{2\text{II}} = .70$; $r_{3\text{III}} = .75$; $r_{4\text{IV}} = .85$, a not unlikely situation, probably more optimistic than otherwise. The first tetrad equation, $t_{1234} = r_{12}\, r_{34} - r_{13}\, r_{24}$, becomes $.60 \times .40 - .50 \times .30 = .09$, and the $PE_{t_{1234}}$ by Kelley's formula (*Crossroads*, p. 49) is .04 (correct to two decimals). If now we correct all of our *rs* for attenuation, the tetrad difference becomes $\frac{.09}{\sqrt{.80 \times .70 \times .75 \times .85}}$ or .15, while its PE increases only to .042, i.e., .04 to two decimals. The uncorrected tetrad difference is barely twice its PE and is hence hardly significant; but the corrected tetrad difference is surely significant, being four times its PE. It can be shown that the PE of a corrected tetrad difference does not increase in proportion to the increase in the tetrad difference. Just what is the relation between the two is not known, but it is certainly not linear. It seems that we are forced to conclude that the unreliability of tests does have a very decided effect upon the tetrad criterion.

In his Section 17, Holzinger deals with the very important question of the effect of heterogeneity in the data upon the tetrad differences. He cites an experiment of his own in which he seems to find that age heterogeneity has little or no effect upon the size of the tetrads. It seems to the reviewer that this conclusion is liable to misinterpretation, as it will probably be taken by the reader to mean by implication that heterogeneity is a relatively unimportant factor and need not be controlled in correlational analysis. Holzinger's experimental data consisted of four measurements, lung capacity, weight, height, and ossification ratio, taken upon a group of 520 boys ranging in age from 8 to 19 years. The elimination of age by partial correlation and in other ways seemed to the author to have little systematic effect upon the tetrad differences; and hence he decides that controlling age does not change the tetrad relationships. Even if we accept this conclusion for physical data at face value—and it seems highly doubtful on several counts—it does not follow, of course, that we can safely disregard age cor-

rections in dealing with mental test data. It is well known that age heterogeneity as well as heterogeneity as to race, sex, and nurture, all tend to produce spurious correlations. These influences must be controlled if we are to uncover the essential similarities or dissimilarities among measured abilities. The worthlessness of much of the correlational results of the past 25 years grows (more than from any one other thing) out of the failure to realize this; and it is unfortunate that Professor Holzinger should—even by implication—decry experimental or statistical corrections for heterogeneity.

In the two concluding sections of his monograph, Holzinger illustrates factor patterns other than Spearman's g and s and considers the effect of overlapping variables upon the tetrad criterion. These topics are clearly explained. Probably the fairest estimate of this monograph as a whole is to say that Holzinger has done a good job in presenting a theory none too strong in many particulars. It seems too bad that like Spearman he usually prefers to gloss over weaknesses rather than to disarm critics by admitting them at the start.

Columbia University. HENRY E. GARRETT.

COMMUNICATION

Results of the New Plan of Election in the Midwestern Psychological Association

In accordance with a vote of the members, the general principle of preferential balloting was adopted by the Midwestern Psychological Association for its recent election. Since the American Psychological Association is considering a change in election procedure, the results have interest for others than the three hundred members of the Midwestern. Five persons had been nominated by a preliminary ballot for president: F. C. Dockeray, Ohio State; C. A. Ruckmick, Iowa; L. L. Thurstone, Chicago; R. H. Wheeler, Kansas; H. Woodrow, Illinois. Voters were instructed to mark these in order of preference.

The Election Committee and the Council considered two plans, both using order of preference, the Hare and the Nanson systems. Under the former, the first count utilizes only the first choices on each ballot. (See Table 1 in which the candidates are listed in order according to the number of first choices received, as shown in Line I.)

TABLE 1

For President: Total, 193; Majority, 97

	Candidates				
	A (Thurstone)	B	C	D	E
I............	63	48	33	28	21
II............	70	52	33	38	..
III............	85	63	..	45	..
IV............	118	73	..	..	..

Since no candidate has a majority, the lowest, E, is eliminated. The 21 ballots which showed first choices for him are now examined for the second choices. That is, since a given voter may not have his first choice, E, we give him a chance to vote for a second choice. As may be seen from line II which gives the result of the second count of the ballots with E eliminated, A got 7 of these second choice votes, B got 4, C none, D 10. As a result, D overtakes and passes C who is now eliminated as low man. Redistributing C's 33 ballots among the remaining candidates proceeds as before. His supporters divide

almost equally in their next preferences between A, B and D and there is still no majority. The 45 votes from D's pile, however, are quite decisively given to A who thus ends with a comfortable majority over B.

The result is the election of the candidate who had a plurality of the first choices, but the rather slight lead at the start is convincingly shown to be no accident by the consistent piling up of second or third choices for Dr. Thurstone. That a first choice rank does not always correspond with the final rank may be seen in the shift from C and D.

Table 2 shows the results in the balloting for two members of the Council. The nominees (in alphabetical order) were: W. F. Book, Indiana; F. Fearing, Northwestern; F. Goodenough, Minnesota; C. R. Griffith, Illinois; J. P. Porter, Ohio University; C. A. Ruckmick, Iowa; Lee Travis, Iowa; R. H. Wheeler, Kansas; H. Woodrow, Illinois. In the table, the candidates are listed according to the number of first choices received.

TABLE 2

193 Ballots. Necessary for Election, 65 Votes

	F (Woodrow)	G (Ruckmick)	H	I	J	K	L	M	N
I	35	31	26	21	19	19	17	13	12
II	42	42	30	29	25	25	..	..	..
III	62	57	35	39	..	..	..	..	..
IV	73	69	..	51	..	..	..	..	..
	(elected)	(elected)							

The three lowest candidates were eliminated after the first count. It may be occasionally advisable to eliminate one at a time. Had it been true, *e.g.*, that those voting for candidate N had preferred L rather consistently, L would thus have had strength enough to stay in the race, but inspection showed that the second choices were scattered. Time was saved, therefore, by transferring, at one time, the ballot of all three of those who seemed most clearly out of the running. A tie on the second count for the bottom position was similarly decided by dropping both low men; in case of need, there are rules governing ties.

The most interesting feature is the slow start and rather strong finish of I. Another feature illustrates a strong point of the system. Two institutions had two candidates each, presumably somewhat dividing the vote from those universities. In both cases, the elimination of one of these candidates led to a large increase for his colleague

upon the next count. In other words, under this system you may safely vote for the candidate of your first choice even though he is likely to be weaker at the poll without feeling that your ballot is wasted; if your first choice goes out, your vote will still be counted for your second.

The Nanson system is not only much more difficult to administer but also harder to explain in detail. The net effect, however, is *as if* each candidate had been compared with every other candidate (as in the method of paired comparisons) and had received a weighted vote accordingly. In this system, a low voting index is favorable. Table 3 shows the vote for president thus computed: the order of candidates is the same as in Table 1.

TABLE 3

A (Thurstone)	B	C	D	E
443	531½	700	584	636½

It will be observed that with a weighted voting system C falls below both D and E, though he had more first choice votes as may be seen from Table 1. The rules require the elimination of all who fall below the average voting index. This eliminated C, D, and E. Second choices are now examined. A has 55, B 24, and A is seen to be elected. Under certain conditions a third count may be necessary.

Table 4 gives similar results for the Council. The candidates are labeled and in the same order as in Table 2.

TABLE 4

	F (Woodrow)	G (Ruckmick)	H	I	J	K	L	M	N
I	778	871½	1021½	977½	975	914	1011	1054	1042
II	367½	383½	..	..	..	407	..	..	..
	(elected)	(elected)							

The average index on the first count is 965. All below this are eliminated. The second computation yields an average of 386 and K is eliminated. The striking difference between the results of the two systems is the poorer showing of H and the improved showing of K under the Nanson system. It is also noteworthy that, had there been three vacancies to be filled instead of two, candidate "I" would have been elected by the Hare system, K by the Nanson. The discrepancy here is not great, however, since "I" was eliminated by a relatively small margin in the latter case. No system, obviously,

can discriminate perfectly when the differences are small. It may clarify the results if we say that K is a person with a relatively small " following " but is an exceedingly active person, professionally, both in general and in the affairs of the Association. It is clear that while few gave him first choice, nearly everyone rated him fairly high.

Of the two systems, the Nanson is the more accurate in obtaining the most representative single person as in electing to the presidency. It is less suitable than the Hare system in the election of more than one person to a multi-member board or council, especially where it is desirable for various interests or factions to be represented. For this purpose, the Hare system is both theoretically and practically much better. Since, however, this is not the situation prevailing to any large extent in either the Midwestern or the American Psychological Association, the Nanson system seems theoretically the better.

It is, however, vastly more cumbersome—and both systems do take a bit of the time of the Election Committee. In most cases, the two systems will yield the same or very nearly the same results. Both are an improvement over plurality voting, if in no other respect, at least in giving all concerned an assurance that the true wishes of the electorate have been closely approximated. Need one be adopted to the exclusion of the other? We think not and submit a draft of compromise rules.

I. The Election Committee shall present for the final ballot, guided by the nominating ballot and by nominating petitions, if any, for each category of office to be filled at least as many candidates as are indicated by the formula (4n—1) where n is the number of coördinate places to be filled. (For example, for President at least five candidates, for two Councilmen at least nine, for three at least thirteen candidates.)

II. After collecting the final election ballots, the Committee shall start by tabulating the first choices. If it is found that twice the number of candidates to be elected have together a sum of first choices equal to, or more than, one-half of the number of ballots cast, the Election Committee shall proceed by eliminating according to the Hare system, the less successful candidates one by one in successive distributions, until those only remain who have the number of votes necessary for election. (This is the case with the vote for President as may be seen from Table 1.)

III. If it is found in the starting tabulation that the (2n) highest candidates (where n as before is the number of coördinate places to be filled) together fail to have first choices equal to half of the number of ballots cast, the Election Committee shall decide that the first choices have been determined excessively by local or sectional considerations. The Committee shall therefore discontinue the Hare system and start anew by the Nanson system.

MAX F. MEYER,
Chairman Election Committee.

HORACE B. ENGLISH,
Secretary, Midwestern Psychological Association.

NOTES AND NEWS

DR. CHRISTIAN A. RUCKMICK, Professor of Psychology at the University of Iowa, has been invited to give a series of lectures during April on a tour of the west and the Pacific coast. The lectures will be on the " Facial Expression of Emotion " and the " Galvanic Technique in the Investigation of the Affective Processes." There will also be a number of conferences with graduate students on the methodology of the affective responses. The institutions visited will include the University of Nebraska, University of Denver, University of Utah, University of Southern California, University of California, College of the Pacific, Stanford University, University of Oregon, Whitman College, State Normal College at Bellingham, Washington, and the University of Montana. On March 20th he lectured at Northwestern University on the galvanic technique.

PROFESSOR K. S. LASHLEY, of the University of Chicago, is spending this quarter in Europe, giving a series of lectures at the University of London.

PROFESSOR J. J. B. MORGAN, of Northwestern University, and Professor Paterson, of the University of Minnesota, will teach at the University of Chicago during the Summer Quarter.

PROFESSOR E. S. CONKLIN, of the University of Oregon, was teaching courses in Psychology in the Divinity School of the University of Chicago during the Winter Quarter, and will return for the Summer Quarter.

Dr. E. Glen Wever has been promoted to an associate professorship and Dr. Charles W. Bray to an assistant professorship in Princeton University.

Professor Franklin Henry Giddings, for many years Professor of Sociology at Columbia, died on June 12 at his home in Scarsdale, at the age of seventy-six.

Mr. Lawrence K. Frank of New York City, at present a staff member of the President's Committee on Recent Social Trends, has been elected Associate Director of Education for the General Education Board. He will complete his present work during the summer and will take up his new duties on October 1. Mr. Frank was for seven years on the staff of the Laura Spelman Rockefeller Memorial and the Spelman Fund, giving his time particularly to the field of child study and parent education. His work with the General Education Board will be in this field and in other areas of education.

On June 15 the degree of Doctor of Science was conferred upon Professor Raymond Dodge of Yale University by Wesleyan University. Professor Dodge was a member of the Wesleyan faculty from 1898 to 1924.

Dr. Clarence Henry Graham, Ph.D. Clark, 1930, has been appointed Assistant Professor of Psychology in Clark University. He has been given a one-year's leave of absence at the beginning of his appointment in order that he may continue as National Research Fellow in the Department of General Physiology at the University of Pennsylvania.

At the first Annual Assembly, May 14–16, of the National Advisory Council on Radio in Education, of which Robert A. Millikan is President, the situation with reference to the broadcasting of psychology was considered in a report of the committee appointed by the American Psychological Association, consisting of Paul S. Achilles, Arthur I. Gates, and Walter V. Bingham, Chairman. This committee is gathering information about the broadcasting of psychology to mature listeners, and would welcome correspondence with psychologists as to their experience with such broadcasting, the difficulties met, and the results achieved. The address of the Chairman is 29 West Thirty-ninth Street, New York.

Dr. Leonard Carmichael, Professor of Psychology in Brown University, has been appointed Visiting Professor of Psychology in Clark University for the academic year 1931–32. Dr. Carmichael will give a graduate seminar in sensory psychology and will be a member of the general departmental seminar.

BOOKS RECEIVED

BARBARA STODDARD BURKS, DORTHA WILLIAMS JENSEN, LEWIS M. TIERMAN, *The Promise of Youth.* California: Stanford University Press, 1930. Pp. v+508.

D. B. LUCAS and C. E. BENSON, *Psychology for Advertisers.* New York: Harper & Brothers, 1930. Pp. vii+351.

MAURICE GARCON and JEAN VINCHON, *The Devil* (translated by Stephen Haden Guest). New York: E. P. Dutton & Co., 1930. Pp. 288.

R. MONEY-KYRLE, *The Meaning of Sacrifice.* London: The Hogarth Press, 1929. Pp. 273.

EDWARD D. SNYDER, *Hypnotic Poetry.* Philadelphia: University of Pennsylvania Press, 1930. Pp. vii+162.

HAROLD BENJAMIN, *An Introduction to Human Problems.* Cambridge: Houghton Mifflin Co., 1930. Pp. vi+472.

OSWALD KROH, *Die Psychologie des Grundschulkindes.* Langensalza: Hermann Beyer & Söhne, 1930. Pp. 352.

CORINNE ROCHELEAU and REBECCA MACK, *Those in the Dark Silence.* Washington: The Volta Bureau, 1930. Pp. 169.

DAVID KATZ, *The Vibratory Sense and Other Lectures.* University of Maine, 1930. Pp. 163.

L. BRETEGNIER, *L'activité Psychique Chez Les Animaux.* Paris: Vigot Frères, 1930. Pp. vi+387.

WELLS P. EAGLETON, *Intradural Conditions in Relation to Rhinology and Otology.* (A critical survey of recent literature.) Chicago: American Medical Association, 1929. Pp. 51.

The Type of High School Curriculum Which Gives the Best Preparation for College. Lexington: University of Kentucky, 1930. Pp. 105.

ARTURO AMEGHINO, *Actas de la Primera Conferencia Latino Americana de Neurologia, Psiquiatria y Medicina Legal.* Buenos Aires: Imprenta de la Universidad, 1929. Pp. 707.

DR. MED. N. E. ISCHLONDSKY, *Physiologische Grundlagen der Tiefenpsychologie unter besonderer berücksichtigung der Psychoanalyse.* Berlin und Wien: Urban & Schwarzenberg, 1930. Pp. vi+356.

DR. MED. N. E. ISCHLONDSKY, *Der bedingte Reflex und seine Bedeutung in der Biologie, Medizin, Psychologie und Pädagogik.* Berlin und Wien: Urban & Schwarzenberg, 1930. Pp. vi+328.

United Fruit Company Medical Department, Eighteenth Annual Report. Boston, 1929. Pp. 451.